Discovering Water

James Watt, Henry Cavendish and the Nineteenth-Century 'Water Controversy'

David Philip Miller

ROUTLEDGE

Science, Technology and Culture, 1700-1945

The 'water controversy' concerns one of the central discoveries of modern science, that water is not an element but rather a compound. The allocation of priority in this discovery was contentious in the 1780s and has occupied a number of twentieth-century historians. The matter is tied up with the larger issues of the so-called Chemical Revolution of the late eighteenth century. A case can be made for James Watt or Henry Cavendish or Antoine Lavoisier as having priority in the discovery, depending upon precisely what the discovery is taken to consist in; however, neither the protagonists themselves in the 1780s nor modern historians qualify as those most fervently interested in the affair. In fact, the controversy attracted most attention in early Victorian Britain some fifty to seventy years after the actual work of Watt, Cavendish and Lavoisier.

The central historical question to which the book addresses itself is why the priority claims of long-dead natural philosophers so preoccupied a wide range of people in the later period. The answer to the question lies in understanding the enormous symbolic importance of James Watt and Henry Cavendish in nineteenth-century science and society. More than credit for a particular discovery was at stake here. When we examine the various agendas of the participants in the Victorian phase of the water controversy we find it driven by filial loyalty and nationalism but also, most importantly, by ideological struggles about the nature of science and its relation to technological invention and innovation in British society.

At a more general, theoretical, level this study also provides important insights into conceptions of the nature of discovery as they are debated by modern historians, philosophers and sociologists of science.

About the Author

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For Margaret

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Preface and Acknowledgements

It must have been about 1963 that I blew myself up trying to make water. Being a nerd just as it was ceasing to be fashionable, I'd persuaded my father to buy me not just a chemistry set but a batch of high-class chemical apparatus from the local 'swap shop'. I'd read about hydrogen and how it burned in air with a blue flame to produce water. Synthesizing water appealed to me and I duly produced hydrogen, using zinc and sulphuric acid, in a small flask with a beautiful, delicate spout. After what I took to be a suitable interval to make sure that all air had been driven from the flask so that it was full of hydrogen, I struck a match and lit the emerging gas … The fact that I survived the explosion was quite remarkable, as the apparatus distributed itself to all corners of the room without any of it connecting with me. My schoolmasters subsequently advised that I should be a chemist. Fortunately I didn't listen to them and now, many years later, I find myself wrestling again with the composition of water but from a safe, historical point of view.

I was embarked all those years ago on a childhood journey of discovery, but I knew that I was not the discoverer. My chemistry books told me that. I had acquired a small library of the kind of texts you could find on the bottom shelf in second-hand book stores, some of which I still have, books by people with quaint-sounding names like Holmyard and Lowry. One of these was Lowry's *Historical Introduction to Chemistry* in the 1926 reprint. This, like many books of its genre and period, told me in no uncertain terms that 'Cavendish (1781) prepares water by burning inflammable air with common air'.[1] I, like generations of schoolchildren, grew up knowing that this discovery belonged to, the mysteriously 'Honourable', Henry Cavendish.

We will see in these pages that the schoolchildren of the twentieth century were the recipients of a sanitized story about the discovery of the composition of water. It was a surprise to me later to learn that there had been a controversy about that question and that another childhood acquaintance from a different field, the 'engineer' James Watt, at one stage was championed as the discoverer. The closure of the water controversy was a complex, protracted business. One could argue, in fact, that it is still not entirely closed at least so far as historians are concerned. I hope in this book to throw some light on why kids like me learned what we did about the composition of water. I hope also to throw some light on the nature of scientific discovery and on Victorian scientific culture.

I owe debts to many people who have helped me in the eighteen months or so during which this book has been researched and written. The hard work of my

[1] T.M. Lowry, *Historical Introduction to Chemistry* 1926, p. 113. The first edition was 1915.

colleagues in the School of History and Philosophy of Science made it possible for me to be away on leave for a whole year once the kindness of the university itself had granted me that leave and supported it financially. Miranda Chan of the School provided indispensable help in making it all happen. During time spent researching in the UK I was privileged to hold a Visiting Research Fellowship at the Institute for Advanced Studies in the Humanities at the University of Edinburgh. At the Institute, the Director, Professor John Frow, and Mrs Anthea Taylor helped to make my stay a most pleasant and productive one. During my time in Sydney the Unit for History and Philosophy of Science at Sydney University kindly hosted me as a Visiting Fellow. At another stage, the Science Studies Unit at the University of Edinburgh made me welcome, for which I am most thankful. David and Celia Bloor, Ivan Crozier, Patricia Fara, Jim Endersby, Malcolm Nicolson and Rona Ferguson, Jeffrey Sturchio and Carole Tansley were generous in making an itinerant researcher feel at home, as, in a rather different way, were my parents Albert and Dorothy, my sister Shirley and the boys, Albert and Eleanor Emmett, and the Hughes family. Seminars at the universities of Aberdeen, Cambridge and Sydney helped me to hone my ideas, as did colleagues in Taiwan during a visit to their National Conference in the History of Science. Thanks to Professor Fu Daiwie for that opportunity.

A historian inevitably makes himself a serial pest in any number of libraries. I owe particular thanks to Peter Asplin and colleagues in Special Collections at the University of Glasgow Library, to Sheila Noble and all the other staff in Special Collections, the University of Edinburgh Library, to Norman Reid at the University of St Andrews Library, to staff at the National Library of Scotland who aided and abetted my study of a bewildering battery of chemistry texts and much else besides, and to staff at the British Library. Joanna Best and others at the Wren Library, Trinity College Cambridge were most helpful, as were staff at the Royal College of Surgeons (Edinburgh) and, not least, in the Interlibrary Loans Section of the Library of the University of New South Wales.

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Apart from my general debt to the scholars whose work I have used, I owe particular scholarly and personal debts to Jack Morrell, John Schuster, David Oldroyd, Rod Home, Simon Schaffer and Richard Yeo. Thank you, gentlemen. Friends in the water at Bondi, Cranbrook and the Des Renford Aquatic Centre helped to keep me fresh and, reasonably, fit. Having the blowtorch of Australian scepticism applied to the belly of a project like this one is a sobering experience! It has made me, as my Bondi mates would say, ‘as dry as a Pommie’s towel’. Finally, special thanks to my wife Margaret for her constant, loving, help and support, in this as in all else. Our children, Kylie and Tom, have shown a suitably vague but cheerful interest in my obsession while providing me with many other reasons, in the water and out, for being proud of them.

Sydney
New South Wales
April 2003

Abbreviations used in Footnotes

Watt Papers	Boulton and Watt Collection and Muirhead Collection, Birmingham Central Library
Watt Papers (Doldowlod)	The Papers of James Watt and his Family (formerly held at Doldowlod House), Birmingham Central Library
Muirhead Papers	James Patrick Muirhead Papers, Special Collections, Glasgow University Library
Brougham Papers	Papers of Henry Brougham, Lord Brougham and Vaux, University College London Library
Whewell Papers	Papers of the Reverend William Whewell, Trinity College Library, Cambridge
Forbes Papers	Papers of James David Forbes, University of St Andrews Library

Chapter 1

Introduction

Initial Orientations

Being first in science is important, as it is in geographical discovery. Entering virgin territory geographically or conceptually is a privilege that few of us will experience. 'Proudly to have thought where none have thought before' might be the scientist's Star Trek experience. However, merely thinking and discovering are a world apart, as we shall see. Discovery is a social as well as an intellectual process. It is, we will argue, a property ascribed to certain intellectual, and practical, processes rather than inherent in them.

This book concerns discovering water, specifically discovering that water is not an element, as had been thought from ancient Greek times until the eighteenth century, but rather a compound. To be able to claim that momentous discovery would be a precious thing. It is perhaps understandable that there was a contest, a priority dispute. What is surprising is how long that dispute lasted and the variety of people drawn into the lists. The contest endured for at least seventy years, from the 1780s into the 1850s. Those in the running to be credited with the discovery were the dour Scottish engineer and improver of the steam engine, James Watt, the aristocratic and eccentric natural philosopher, Henry Cavendish, and the French chemist, tax farmer and victim of the revolutionary guillotine, Antoine Laurent Lavoisier. They took relatively little part in open priority dispute themselves. It was in the 1830s through to the 1850s, when the original protagonists were long dead, that the 'water controversy', or the 'water question' as it was often called, reached its highest intensity. I am interested to understand why this was so.

It will be useful to establish some basic features of the controversy's chronology and *dramatis personae*. The water question reached its peak in the aftermath of the 1839 meeting of the British Association for the Advancement of Science in Birmingham. That meeting was a small one because it was held amidst Chartist agitation. Those who attended were deliberately marking the relationship between science and industrialization. The Association had met since its founding in 1831 in major academic venues, regional centres and commercial ports, but this was a highly symbolic meeting in a primarily manufacturing town. The President-elect of the Association at Birmingham was the Reverend William Vernon Harcourt, the Oxford-educated son of the Archbishop of York. Harcourt had been one of the founders of the Association in 1831 at its first, York, meeting.

On the evening of Monday, 26 August 1839, Harcourt delivered a discourse to the Association's General Meeting in Birmingham Town Hall. Touching on a variety of topics, Harcourt addressed himself to the water question. Specifically, he took issue with claims published by the Perpetual Secretary of the French Académie des Sciences, François Arago, shortly before, in his long-delayed *Eloge de James*

Watt. Arago, aided in production of the *Eloge* by Watt's son, James Watt Jr, and by the prominent lawyer and politician, Henry, Lord Brougham, had made strong claims that Harcourt took issue with.[1] Arago claimed priority in the discovery of the compound nature of water for James Watt. Even more contentiously, he claimed to show, with the aid of Brougham's investigations, that the other British claimant to the honour, Henry Cavendish, had taken Watt's ideas as his own. Harcourt defended Cavendish's claim to priority and rebuked those who cast aspersions on his honesty. Although Harcourt tried to give due recognition to Watt's steam-engine improvements, the reception of his 'Address' was mixed. Many were aghast that Harcourt should take this occasion to launch what they construed as an attack upon a local hero who symbolized for many that union of science and industrial development that the Birmingham meeting was supposed to celebrate.

The 1840s saw a spate of publications on the water question as Harcourt and his supporters among the 'Gentlemen of Science' of the Association squared off against Arago, Brougham, James Watt Jr and Watt's relative and recruit as family historian, James Patrick Muirhead. Pamphlets and books became the occasion for long essays in the various reviews through which so much intellectual debate in early Victorian Britain took place. In 1851, a Scottish chemical lecturer, Dr George Wilson, published his *Life of the Honourable Henry Cavendish*. A peculiar biography, this work was largely devoted to a thorough dissection of the water question and the literature that it had generated up to that point. In the following decades an orthodoxy emerged that gave the palm to Cavendish.

A similar sort of orthodoxy had emerged in the period between the 1780s and the publication of Arago's *Eloge*. The dispute between the protagonists themselves had been brief and mild compared with what was to come later. The term 'water controversy' was not employed in the 1780s, but the question of the discovery of the composition of water was of great moment. Many people perceived it as a key event in the battle between the supporters and opponents of the chemistry of phlogiston. Precisely what happened during the combustion of inflammable air was in dispute. The answer that was eventually accepted and became part of the new system and language of chemistry associated with the name of Lavoisier, was, of course, that 'hydrogen' burned with 'oxygen' to produce water as a compound of those two gases. Lavoisier's claim to the discovery was challenged, especially in Britain by supporters of Cavendish. Watt, too, was sometimes given credit for having important ideas on the subject. On the whole, however, by the 1790s Cavendish was regarded as the discoverer, in Britain at least.

Returning to the rationale for this study, it might be considered that the second phase of the water controversy is a peculiar topic for book-length examination. Even the first phase has not received substantial modern treatment, being very briefly dealt with, for example, in Jan Golinski's excellent study of the public culture of chemistry during the relevant period.[2] This itself is perhaps an indication that historians have thought the water question to be somehow unworthy of treatment, as a 'storm in a teacup'.

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[1] Throughout, 'Watt' refers to the famous engineer, 'Watt Jr' to his son James.

[2] Jan Golinski, *Science as Public Culture. Chemistry and Enlightenment in Britain, 1760–1820*, 1992, pp. 133–37.

The fact that Watt and Cavendish themselves remained rather cool about the issue, at least in public, means that the second phase of the controversy is easily seen as a minor imbroglio driven by Watt Jr's filial concerns, Scottish nationalism, and Arago's and Brougham's political agendas. This perspective is doubly attractive if one is convinced, as most modern historians appear to be, of the solidity of Cavendish's claim and the tenuousness of Watt's. The recent biographers of Cavendish, Jungnickel and McCormmach, were wary of the controversy because in their view (which has much going for it) the historical picture of Cavendish has been severely distorted by the undue attention given to the water controversy.[3] Although Wilson's biography vindicated Cavendish's claim, it neglected to develop many other aspects of his life and in some ways painted a rather jaundiced picture of the man. Having discussed the first phase of the controversy, Jungnickel and McCormmach offer the following observation:

> A second water controversy arose long after the participants in the first were dead. It was prompted by the Secretary of the French Academy D.F.J. Arago, who in his *éloge* of Watt asserted that Priestley was the first person to prove that air could be converted into water and that Watt was the first person to understand it. The consequent furor initiated by Harcourt's presidential address at the British Association meeting in 1839 was sustained by a passion of another kind, nationalism. Since the revived controversy was the occasion for Cavendish's unpublished scientific work to begin to be made public, it had that value if perhaps no other.[4]

Clearly, in writing this book I am asserting that, on the contrary, the water controversy is a worthy and a useful topic. I am also arguing that there was much more driving it than competing nationalisms pitching the Scot versus the Englishman. At the simplest level, the sheer 'air time' that the controversy received, especially in its second, Victorian, phase, makes it worthy of study. We need to be curious about what our forebears found important about this issue, however trite it may seem to us now. More technically, study of the water controversy usefully documents an example of a long-delayed priority dispute. Robert Merton argued many years ago that priority disputes in science are a vital element in understanding the nature of science and of the scientific community. Priority disputes were particularly revealing, Merton suggested, when they were conducted not by the immediate claimants to priority but by others who, apparently, had little to gain personally from the settlement of the controversy. Merton's contention was that such priority disputes are important because they reveal the normative structure of science and how it drives the scientific community.[5]

I share Merton's view that such episodes are revealing, but my account of why they are so differs markedly from his. My account is based in a philosophy and

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[3] Christa Jungnickel and Russell McCormmach, *Cavendish. The Experimental Life*, 1999, pp. 10–14. Although I disagree with the authors on the importance and character of the second phase of the water controversy, I have benefited enormously from their work.

[4] Ibid., p. 380.

[5] See Robert K. Merton, *The Sociology of Science. Theoretical and Empirical Investigations*, 1973, pp. 291–93. Merton, though finding the water controversy important and revealing, nevertheless described it as 'the most tedious and sectarian' on the calendar of eighteenth-century disputes! (p. 288).

sociology of science that rejects the kind of universal normative explanation of scientific action that animates Merton's scheme and gives his approach much in common as an explanatory strategy with universalist philosophies of science. The water controversy is not in my view a secure basis for discerning how scientific communities in general operate. It can be used, as I use it, to support what is called a 'finitist' account of scientific practice and to illustrate a specific view of scientific discovery.[6]

Far from being merely an object lesson in the pernicious influence of nationalism in science, the controversy is a window onto the cultural politics of early Victorian science. The fact that the controversy loomed large for many members of the early Victorian scientific community, the way that they divided up on it, and how they argued their cases are all indicative of deep currents and major emergent structures in early Victorian culture. These features of the controversy feed into larger debates about the nature of science, about the relationship between science and technology and economic transformation, about the appropriate organization of scientific activity, and so on. This is why, in my view, the water controversy is worth studying.

James Watt (1736–1819) and Henry Cavendish (1731–1810) were iconic figures in Victorian culture.[7] Though their significances were many and varied, as a first approximation Watt represented a close liaison between science and technology, one, moreover, unmediated by a university-trained scientific élite. Watt's accomplishments demonstrated that it was possible for the intelligent autodidact to rise to great things, including, perhaps, philosophical insight and scientific discovery. Men such as Watt combined science and action to promote industrial development. Many members of the industrial middle class regarded them as scientific heroes to be revered and emulated.

On the other hand, the emergent, university-trained, and increasingly specialized scientific élite considered too much popular adulation for Watt as 'philosopher' or 'scientist' to be dangerous. As that élite sought to negotiate their relationship with governments and wider publics, they were cautious about portraying too close a link between science and practical utility. They needed support for sustained trains of research aimed primarily at understanding the natural world. Only if such support was forthcoming would the real benefits of utility eventually flow, they argued. This was an argument on the basis of 'ultimate utility' but also on civilizational grounds. For the scientific élite Cavendish was a more apposite icon

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[6] The single best critique of Merton along these lines is Michael Mulkay, 'Interpretation and the use of rules: The case of the norms of science', *Transactions of the New York Academy of Sciences*, Series 2, **9**, 1980, 111–25. On 'finitism' see Barry Barnes, David Bloor and John Henry, *Scientific Knowledge. A Sociological Analysis*, 1996, pp. 54–80.

[7] There is an enormous literature on James Watt, but the interested reader might usefully enter it via two recent publications: the first volume of a major new biography, Richard L. Hills, *James Watt. Volume 1: His Time in Scotland, 1736–1774*, 2002, and a delightful introductory essay by Ben Marsden, *Watt's Perfect Engine. Steam and the Age of Invention*, 2002, which is reliable and has an excellent section on Watt's reputation and iconic status. Christine MacLeod is undertaking an important study of Watt's nineteenth-century reputation, especially among the industrial classes, as part of her book in progress entitled *Heroes of Invention: Celebrating the industrial culture of nineteenth-century Britain*.

than Watt. Cavendish represented in their estimation a person who pursued a methodical, cautious, sustained train of research. This research was of the highest quality and driven by curiosity alone. The highest standard was reached precisely because the research was pursued in this manner. Ultimately, much rested upon the outcome of the contest between these competing icons and ideologies: relations between science and government; the organization and hierarchy of science education; and the organization of scientific institutions. In this sense, as a historian, to enter the water controversy and merely seek to arbitrate it is in my view to miss the point.

A key feature of this book is that I do not seek to resolve the water controversy. In terms of modern controversy studies I seek a 'symmetrical' approach. I aim to understand and explain the stances taken and arguments involved, not judging their relative value. Given that the modern scientific, and historical, consensus is that Cavendish discovered the compound nature of water, many readers will find a symmetrical approach disturbing. This is because when credit has already been distributed, to pursue a symmetrical approach is automatically to challenge the *status quo*.[8] I give the case of Watt and his supporters more credit than most people would consider they deserve. I do this not by explicitly siding with the Watt camp but just by taking them seriously. This approach, however, offers advantages to historical understanding.

Suppose that my stance were more of a 'realist' one – that there were a *correct* answer to the question 'Who discovered the composition of water?' Suppose that I then set out to ascertain that correct answer through historical research devoted entirely to the actions of the original protagonists in the 1780s. Where would this place me in relation to the Victorian water controversy? I would end up throwing my weight on one side of the balance, the side that was right (or most right) in my view. Their understanding, in so far as it coincided with mine, would require no further explanation. They would have simply got the history right. My task in that situation would be to explain why those who got the history 'wrong' were diverted from the truth. The problem with this, of course, is that my historical research into what happened in the 1780s cannot stand outside the controversy itself. I will bring philosophical conceptions to the task that will align me in the controversy. In particular, I will have to bring to the task conceptions of what it means to discover something. If my conceptions coincide, let us say, with those maintained by the supporters of Cavendish, then it is not surprising that I end up sharing their view of the identity of the discoverer. My task, then, is not only to describe the competing discovery accounts offered by the supporters of Watt and Cavendish, but also to bring out the underlying, competing, criteria of what constitutes discovery. This allows me to build plausible connections between substantive positions taken in the priority dispute, different stipulations about what the criteria of discovery should be, and the interests that sustained them. In this way the nature, course and

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[8] For the original statement of the principle of symmetry see David Bloor, *Knowledge and Social Imagery*, 1976. A relevant exchange on symmetry, neutrality and capture of the analyst occurs in Pam Scott et al., 'Captives of controversy: The myth of the neutral social researcher in contemporary scientific controversies', *Science, Technology & Human Values*, **15**, 1990, 474–94, the response by Harry Collins, idem, **16**, 1991, 249–51 and the rejoinder, idem, **16**, 1991, 252–55.

significance of the water controversy in early Victorian scientific culture can be better understood. This understanding does not depend on resolving the original controversy and, I argue, is best served by not attempting such a resolution.

The Structure of the Story

The theoretical stances that inform my approach are delineated in Chapter 2 and are based in the sociology of scientific knowledge (SSK). Underlying the work of the 'Edinburgh School' in SSK and of my own approach is a 'finitist' account of knowledge and meaning. This is a quite general account applicable to the terms of scientific discourse, including such concepts as 'scientific discovery'. Finitism radically contextualizes those terms. When applied to the notion of 'discovery', finitism leads us to an 'attributional' model of the process. This model was admirably developed by Augustine Brannigan many years ago in a way that has yet to be significantly improved upon. Work from within the Edinburgh School, notably by Barry Barnes, made clear the links between finitism and the attributional model.[9] This chapter also describes the rhetorical analysis of discovery accounts, which, according to some, should supplant SSK-based accounts. I argue that, on the contrary, these rhetorical approaches are a useful supplement to the attributional model based in finitism, not a viable substitute for it, at least if our aim is historical explanation. Although in some senses distracting from the historical narrative, much of which can be read without serious concern for these deeper philosophical questions, Chapter 2 underpins both the general approach taken and the detailed historiography pursued.

One consequence of my SSK-based approach is that Chapter 3, dealing with the period in the 1780s when the original scientific, or natural philosophical, work was done, has self-imposed limitations. For reasons already given, this chapter cannot be, and does not attempt to be, an account of who *really* discovered the composition of water. The aim is rather to survey those 'happenings' that became the subject of controversy. Maintaining neutrality in doing this is a strain. For I can no more excise myself from existing cultures of interpretation than could the historical actors whom I study. I can, however, highlight pivotal points about which the Victorian water controversy was to turn. Apart from telling the 'story of water' in the 1780s in this way, this chapter also surveys the historiography of the Chemical Revolution, the part played in it by the discovery of the composition of water and seeks to contextualize the chemical investigations of Watt and Cavendish.

Chapter 4 provides the first of two 'attributional surveys'. Finitism leads us to the view that meaning is a matter of usage. So too is the attribution of discovery. The attributions made by protagonists in the controversy are really only the tip of an iceberg. The rest of the iceberg is composed of a myriad of attributions made during the course of everyday activities and other exchanges. The water controversy, the nature of water, the work of Watt, Cavendish and Lavoisier are all topics that were (and are) dealt with in various types of surviving literature. Among the least

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[9] See Augustine Brannigan, *The Social Basis of Scientific Discoveries*, 1981 and Barry Barnes, *T.S. Kuhn and Social Science*, 1982, pp. 41–45, 94–101.

diffuse of these, and therefore reasonably accessible, are accounts that appeared in encyclopaedias, dictionaries and textbooks. These reached a far wider audience than the documents of explicit controversy. They were also, of necessity, constrained to be brief and to distil the outcomes of more complex accounts.[10] Any act designed to change meaning, and to change minds, faces the constraints of prior structured belief. Chapter 4 is an attempt to characterize the prior structured belief that was faced by those in the Watt camp who sought to re-open the 'water controversy' in the 1830s.

Most prominent within that camp was the great engineer's son, James Watt Jr. Chapter 5 is the first of the central sequence of chapters (Chapters 5–9) analysing the involvement of various individuals and groups in the controversy and it deals with the younger Watt. Some have considered that the Victorian water controversy requires little explanation other than the impetus given to it by filial piety. It is certainly true that the controversy was kicked along by what I call the 'filial project', with its various textual, monumental and other commemorations of Watt. I examine Watt Jr's own life and preoccupations. Watt Jr was certainly very jealous of his father's reputation and he sought to propagate a particular image of his father. Watt Jr's approach to the water controversy was shaped by his intensely empirical mindset that was reflected in his accounting practices within business. Keeping account, for Watt Jr, whether in matters of business or reputation, was an enduring feature of Enlightenment rationalism. We will see that his approach to discovery is assimilable to one of the major stances identified in Chapter 2, and yet the origins of that approach in his case were clearly culturally specific. Although Watt Jr's push was primarily filial, he did have views about scientific and business development and so participated in the wider ideological uses that the industrial middle class made of his father's reputation.

Watt Jr's written contributions to the water controversy were not substantial. However, he was constantly behind the scenes of others' efforts. This was so in the case of François Arago, whose key role in the controversy is the focus of Chapter 6. A later Secretary of the Académie des Sciences, Berthelot, was to take a nationalistic line in the late nineteenth century in reclaiming the discovery of the composition of water for Lavoisier.[11] Arago's nationalism did not take that form. He relegated Lavoisier to the background, as did most participants in the Victorian water controversy, and considered the rival claims of Watt and Cavendish. Though heavily prompted by both Watt Jr and Lord Brougham, Arago had his own reasons for wishing to laud Watt and to cast his vote against Cavendish. I argue that these reasons had to do with Arago's radical political stance regarding both French industrial development and the conduct of the Académie des Sciences. Arago found the example of Watt useful in arguing for closer linkages between the science of the Académie and technology for industrial and commercial development. Also, Cavendish's sometimes non-communicative behaviour and his reputation as a strangely aloof character did not endear him to Arago, who was engaged in a

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[10] In the terms developed by Latour and Woolgar, such accounts are obliged to drop modalities and to become more 'fact-like'. See Latour and Woolgar, *Laboratory Life: The Social Construction of Scientific Facts*, 1979, pp. 75–86.

[11] M. Berthelot, *La Révolution Chimique, Lavoisier*, 1902 (first edition 1890), pp. 109–33.

campaign to open the communications of the Académie to greater public scrutiny. So we see once again the varied interests behind attempts to revive the claims of Watt. Arago's scientific credibility was most important to the Watt cause.

Science in Britain was undergoing a major organizational and leadership transformation in the 1830s as Arago prepared, delivered and published his *Eloge de James Watt*. Whilst the reform movement in the Royal Society of London had formally failed when John Herschel was defeated for the Presidency by the Duke of Sussex in November 1830, in practice a new leadership was emerging. This leadership transcended the old, paternalistic and aristocratic structures of the 'Banksian Learned Empire'. A key development was the founding of the British Association for the Advancement of Science (BAAS) in 1831. Although initially the product of a provincial thrust, the BAAS was rapidly suborned by a university and metropolitan élite, Morrell and Thackray's 'Gentlemen of Science'.[12] These developments provide the crucial context for the phase of the water controversy recounted in Chapter 7. This phase turns on Harcourt's 'Address' to the 1839 Meeting of the Association in Birmingham.

Harcourt's 'Address' is interesting not only for its explicit contribution to the controversy but also for the kind of argument that is made in it. In its published version, at least, this 'Address' took the sophistication of the discussion of discovery to a new level. The defence of Cavendish's claim is mounted via the other main vehicle discussed in Chapter 2, a heuristic argument that involves inferring Cavendish's claim to the discovery from a close examination of the train of research in which he was involved. This was the approach to be taken by most of those involved with the leadership of the Association.

Not all the prominent scientists in Britain, however, were entirely enthusiastic about the BAAS and its conduct. John Herschel was rather lukewarm about it. Charles Babbage and David Brewster were at times openly hostile. A key target of this hostility was William Whewell. We see in Chapter 7 that Whewell was very much behind the scenes as Harcourt developed his arguments. The water question had entered an ideological maelstrom of competing ideas about the nature of science, how it should be supported, conducted and organized, and how it should relate to the industrial arts, or as we would say, 'technology'. Whewell was at the centre of this debate, as was his chief critic David Brewster. Brewster's involvement in the water question is also a primary concern of this chapter.

In Chapter 8 we return to the Watt camp and examine the 'advocates of Watt' – Henry Brougham, Francis Jeffrey and James Patrick Muirhead. These gentlemen, Scottish lawyers all, took the Watt camp's fight into the 1840s and beyond, as Watt Jr's health began to fail before his death in 1848. Brougham had long been involved with the making of Watt's reputation. This chimed in well with his efforts to promote Mechanics' Institutes, adult education and 'Useful Knowledge'. In the 1830s and 1840s the former Lord Chancellor was something of a loose cannon in scientific affairs as well as in politics. We trace his relations with the emergent scientific élite as another stimulus to Brougham's heterodox views. Brougham's old friend, co-founder of the *Edinburgh Review* and fellow judge, Francis Jeffrey

[12] Jack Morrell and Arnold Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science*, 1981.

had long remained very much on the periphery of the water question, even though he had been as proud a custodian as any of James Watt's reputation. Jeffrey was drawn into the water question via the affairs of the *Edinburgh Review* and ended up writing one of the most detailed pro-Watt articles for that *Review* in 1848. We will see that Jeffrey and Brougham together pursued an empirical account of discovery, strongly reminiscent of legal pleading in its reliance on the evidence of circumstance. Muirhead completes the trio of lawyers. As the person anointed by Watt Jr to pursue family history and the fight for his father's reputation, Muirhead devoted considerable time and effort to the publication of Watt's *Correspondence* on the water question. His arguments are analysed also for their strategies. The way in which the advocates of Watt cast their arguments was intended to make the water question analogous to a legal issue and not a scientific one. As outsiders to the emergent scientific community, it was clear to them, as to others, that if the issue were fought on scientific terms, then their credibility would be hard to sustain.

We then turn to those who carried forward the Cavendish cause. The character of Cavendish – what our controversialists called the personal and the scientific character of the man – was a potent ideological resource. We examine the general ideological importance of Cavendish to the 'Gentlemen of Science' of the British Association. Cavendish was in many ways a 'live' force in the science of the 1840s, especially with regard to his reputation for precision. This was a value at a premium in early Victorian scientific culture. For the first time in the 1840s it becomes possible to speak of an organized discipline of chemistry. However, chemistry was regarded by many as only just emerging into full scientific status. Identification with the scientific style of Cavendish was useful to the chemists in the quest to gain, and retain, that status. Certainly from this number emerged the most important chemical writer on the water question, Dr George Wilson. We examine Wilson's involvement in the controversy and his arguments (in *The Life of the Honourable Henry Cavendish*) for awarding the discovery to Cavendish. Like Harcourt, Wilson pursued elaborate hermeneutic arguments often involving complex chemical knowledge.

Having laid out the argumentative strategies and the driving interests of the various individuals and groups involved in the controversy, Chapter 10 shifts into a diachronic account of the interactions involved in the controversy during the key decades of the 1840s and 1850s. The focus here is upon evolving mutual perceptions and changing stances.

By the 1850s, the controversy had reached a turning point. It was clear that the efforts on behalf of Watt had largely failed among scientific specialists. The 'closure' of the dispute, however, though dependent primarily on such expert opinion, was ultimately an attributional matter. In the final substantive chapter before the conclusion, I present a second attributional survey, looking once again at the way in which Watt, Cavendish and the water question were dealt with in encyclopaedias and textbooks, this time in the mid- to late nineteenth century. The 'knowledge' that Cavendish was the discoverer of the composition of water was established among scientists, students and the wider reading public through these processes. The threads are pulled together and conclusions are drawn in the final chapter. By this time, it is hoped, the water question's importance to early Victorian scientific culture will be apparent and a fillip will have been given to attributional approaches to the understanding of scientific discovery.

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Chapter 2

The Nature of Discovery: The Attributional Model

The making of discoveries is what science is all about, the stuff of the scientific career and of scientific reputation. Stories about science told at every level, from the most austere philosophical accounts to the heroic tales recounted in the recent 'Sobel Effect' literature, turn around the notion of scientific discovery.[1] The aim of this chapter is to discuss various understandings of scientific discovery and the approach to be taken in this book.

Discoveries as Found Objects or as Ideas Successfully Passing Philosophical Tests

Virtually all accounts of scientific discovery (and of discovery in general) treat it as a process of revelation. We discover 'things', and those things, whether objects or processes, are taken to subsist in nature independently of our knowledge of them. Discoveries are, I suggest, usually conceptualized as objects in a landscape. The landscape pre-exists and human scientific activity either enables us to uncover (literally *dis*cover) objects in that landscape or it does not. Treating discoveries as found objects is, of course, to adopt an essentially realist view of the relationship between the meanings of science and the external world. In so far as realist epistemologies are qualified or dropped, then rationalist views usually take over. Correspondence notions of truth, of the match between scientific discovery and found objects, tend to be abandoned in favour of more tenuous notions of coherence. On this view, the landscape of possible discoveries does not directly determine our uncovering of them, but it does still constrain decisively what we can say about nature and in that sense limits the discoveries that are available to be made.

Philosophical approaches to discovery have divided on the question of the existence of a 'logic of scientific discovery'. Inductivist philosophies have generally been happy to treat their versions of scientific method as providing such a logic. Modern approaches that claim to achieve machine-based discovery are the most radical versions of inductivism in this connection. The family of hypotheticalist philosophies, such as falsificationism, are built upon a distinction between a 'context of discovery' and a 'context of justification'. Despite his talk of 'the logic' of

[1] See Thomas Nickles, 'Discovery', in R. Olby et al. (eds), *Companion to the History of Modern Science*, 1990, pp. 148–65 and David Philip Miller, 'The Sobel effect', *Metascience*, **11**, 2002, 185–200.

scientific discovery, Karl Popper remained adamant that the processes of discovery, in the sense of the psychological processes enacted by the discoverer, are inaccessible to logical inquiry and essentially ineffable. This logical inaccessibility of the processes producing discoveries does not signify much, in Popper's scheme, because until the ideas thrown up by scientists have negotiated the context of justification they cannot be dignified with the title 'discovery' anyway. For Popper, then, to have made a discovery is to have propounded a theory that has survived attempted falsification at some level. As Popper himself put it, in this way we may discover who has made a discovery.[2] Hypotheticalism, then, rejects the idea of a logic of discovery, leaving the process whereby theories are arrived at as the province of the 'irrational', that is, the psychological, sociological or historical. Such philosophies do, however, typically provide criteria of 'scientificity' which a new theory, or idea, or experimental finding has to meet in order that it be recognized as a 'discovery'.

We can usefully tag this set of approaches in their entirety as philosophical models of scientific discovery. There are as many of these models as there are candidate philosophies of science. These models represent a significant departure from the found objects view of discovery although those who adhere to such philosophical models usually, and reasonably, adopt realist language about discoveries in situations where the philosophical filter is considered to have done its work effectively. Proponents of philosophical models do vary, however, in the degree to which they insist upon a *logical* account of discovery. Covering law-style accounts of scientific explanation or even just 'good reasons' for holding scientific beliefs provide alternative philosophical stances. Much historical work on scientific discovery from within 'internalist' traditions in the history of science has been conducted on the basis of these looser constructions of the philosophical basis of sound science. Nevertheless that historical work does retain a philosophical criterion of 'scientificity' as part of its own structure.

Discoveries as Sociological Process: The Attributional Model

However they may vary, philosophical models of discovery rely for their credibility upon the perceived strength of the philosophical account of scientificity upon which they are based. In rejecting the possibility of scientific method, or at the least radically contextualizing it, the sociology of scientific knowledge has therefore provided a rather different, and sometimes quite contrary, set of conceptions of scientific discovery and what it means to study it historically.

Thomas Kuhn heralded the appearance of *The Structure of Scientific Revolutions* with a famous article on discovery in which he argued that there are two classes of discovery.[3] One class is predictable and relatively easy to specify in terms of who did what, when and where. The other class he regards as impossible to specify in those terms. The two types of discovery occur respectively, of course, in what Kuhn was to call 'normal science' and 'revolutionary science'. The consequences of Kuhn's analysis of scientific process for ideas about discovery depend ultimately

[2] Karl Popper, *The Logic of Scientific Discovery*, 1959, p. 31.

[3] T.S. Kuhn, 'Historical structure of scientific discovery', *Science*, **136**, 1962, 760–64.

upon the interpretation of Kuhn that is adopted. A very influential interpretation is that which takes the seeds of conventionalism in Kuhn and drives them towards a full-blown constructivism so far as scientific knowledge, and the terms used to describe that knowledge (including 'discovery'), are concerned. Two key authors in this process have been Augustine Brannigan and Barry Barnes, who have developed the attributional model of scientific discovery.

Barnes arrives at the attributional model of discovery as a particular outcome of the general philosophy of 'finitism' which he sees Kuhn as having pioneered in relation to science. The problem with Kuhn, however, was that, having perceived the non-viability of a rule-governed account of science during major intellectual transformations (paradigm shifts), he retained such an account in normal science. Whereas some scholars sought to repair Kuhn's irrationalism by painting his revolutionary science as normal, others sought to enshrine his constructivism by painting his normal science as revolutionary. This is what Barnes does by placing finitism at the centre of *all* scientific activity. Finitism is an understanding of concepts and concept application and use that derives ultimately from Wittgensteinian ideas about meaning and rules. It is a quite general set of propositions that applies to all concepts, be they everyday, scientific or metascientific ones such as 'experiment', 'discovery' and so on. The easiest way to depict finitism is by reference to its opposite, 'extensional semantics'. According to 'extensional semantics' concepts have predefined extension: all things in the universe are either 'A' or 'not A'. The concept A has a predetermined set of things in it. We may have identified some of them correctly, but not others. Any new instance is placed into the appropriate category according to a determinative set of rules. Philosophical models of discovery essentially provide a set of putative rules according to which instances of scientific activity can be securely identified as obeying or not obeying those rules, and therefore as 'discoveries' or 'not discoveries'.[4]

Finitism treats concepts, on the contrary, as the ongoing product of contingent judgements of similarity and difference. No set of determinative rules is available. Any rule that we may try to use to make establishment of similarity and difference a logical matter, or something that any reasonable person would *have* to admit, is liable to break down, especially if there are incentives for some people to interpret that rule differently. Philosophical criteria are interpretable in divergent and yet still legitimate ways, and so no set of rules as advocated and applied by historical actors can be determinative in identifying one activity rather than another as a 'discovery' while there are people determined to disagree. Any set of criteria used by historians to identify discoveries will be subject to the same limitation.

Barnes uses the example of the discovery of Neptune to illustrate some of these points about the nature of discovery. In the mid-1840s there was great excitement when Adams in Britain and Leverrier in France both claimed discovery of a new planet. Calculations had been made of an orbit for an undiscovered planet *x* that would produce the known perturbations in the orbits of the known planets. Then observers located the object in the sky. Although there were disputes in Europe about who should get the credit (Adams or Leverrier), there was consensus that one

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4 Barry Barnes, *T.S. Kuhn and Social Science*, 1982, pp. 41–45. Augustine Brannigan, *The Social Basis of Scientific Discoveries*, 1981 arrives at a similar position via ethnomethodological precepts.

of them had made the discovery. From the United States, however, came dissenting voices. It was claimed that there was a discrepancy between the predicted orbit and the observed orbit of the optical object. Therefore, the Americans claimed, Adams and Leverrier had actually predicted a *different* planet from the one that was discovered with the telescope. There was an unwillingness to accept this in Europe, it being argued that there would inevitably be some margin of error, some difference between the prediction and the observation. The key question was: what divergence between them was acceptable? Where do we draw the line between a discrepancy so large that it invalidates the putative discovery and one that is within acceptable limits? In the end this was a matter of judgement, and the question was resolved by who could make their judgement stick within the community.[5]

The point is that Adams's and Leverrier's activity represents something that may be placed in the category 'discovery' or 'not discovery'. A judgement has to be made about whether this instance is the same as, or different from, other instances already classified as discoveries. If there is incentive to do so, then any particular judgement can be challenged quite logically; that is, the challenge can be made out as compliant with a given set of rules. Another point to note is that the discovery and the criteria by which it is judged to be a discovery become 'true' together.

In the case of the water controversy there were a number of accounts of what the discovery consisted of. These might be listed crudely as follows:

1. Obtaining water in an apparatus after the explosion of a gaseous mixture (that we now know to have involved a combination of hydrogen and oxygen rather than a condensation of moisture from the gases but that the historical actors didn't so explain)
2. Obtaining water and explaining its production as the result of a chemical combination of two gases
3. Obtaining water and explaining it as the product of the combination of inflammable air and dephlogisticated air, or, obtaining water and explaining it as the product of the combination of inflammable air and dephlogisticated air involving 'elementary' heat
4. Obtaining water and explaining it as the product of the combination of hydrogen and oxygen
5. Obtaining water and explaining it as the product of the combination of hydrogen and oxygen and showing how that reaction related to the whole series of cognate reactions in the New Chemistry of Lavoisier.

The list could be lengthened in various ways by imposing other criteria, for example by saying of (2), (3), (4) and (5) that they must be part of a sequence of demonstrative, quantitative experiments. Depending upon how the discovery was defined, and how the criteria for counting as a discovery were set up, cases could be made for a variety of individuals as the 'true' discoverer.

The controversy over priority of discovery thus involves a number of individuals and groups trying to make the case for their version of the discovery, their criteria of

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[5] Barnes, *T.S. Kuhn and Social Science*, pp. 94–100.

discovery and their discoverer. They make attributions and promote criteria according to their interests and objectives. Eventually some attributions win out and the nature of the discovery and the identity of the discoverer are established as a consensus within the community. In achieving that consensus the history and character of the discovery are typically recast in ways that depict it as an event at which the discoverer was uniquely present. Rhetorical accounts of discovery come to appear non-rhetorical. An event is 'naturalized' and a discoverer reified.[6] This, then, is the attributional model of discovery. It is a controversial model, first, for all those who believe in the possibility of some objective basis for scientific knowledge and who adopt extensional semantics as their theory of how concepts relate to the world. It is also a controversial model for those who feel that, rather than going too far, it does not go far enough in treating discoveries as literary and rhetorical accomplishments.

The most radical account of scientific discoveries within the field of science studies has been that offered by discourse analysis on the one hand and by actor-network theory on the other. Both these approaches challenge the conventional sociology of knowledge by claiming to undermine the sociologists' use of realist sociological categories such as 'interest', or indeed 'society'. They claim to take us back to statements 'pure and simple' or to networks of associations that avoid the reifications of traditional sociological categories and even the presumptions of distinctions between human and non-human actors. On this view, attributional approaches to discovery such as mine are contradictory because they relativize the concept of discovery but then rely on realist sociological language to explain why certain attributions stick and others do not. Such critics say that all we can have are discovery accounts. The analyst's task is to show how those accounts are 'accomplished'.[7]

This last approach involves, in my view, pseudo-empiricism, claiming to be able to access discovery accounts somehow free of context. It involves either abdicating the task of explanation or, in so far as explanations are pursued, smuggling sociological terms in via the back door again.[8] But it can be a useful corrective to over-eager generalization. We need to be reminded of individuals and 'contexts' in continual movement. If scepticism about sociological categories is applied for its own sake, however, the possibilities of historical explanation seem to dissolve before our eyes.

There is, however, another sense in which the discourse analysis approach is useful. It draws our attention to the structures of argument involved in attributional

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[6] For a pioneering case study of these processes see Augustine Brannigan, 'The reification of Mendel', *Social Studies of Science*, **9**, 1979, 423–54.

[7] See as key documents in this tradition: Steve Woolgar, 'Discovery: Logic and sequence in a scientific text', in K. Knorr, R. Krohn and R. Whitley (eds), *The Social Process of Scientific Investigation*, 1980, pp. 239–68; G. Nigel Gilbert and Michael Mulkay, *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse*, 1984; Bruno Latour, 'Give me a laboratory and I will raise the world', in K. Knorr-Cetina and Michael Mulkay (eds), *Science Observed*, 1983, pp. 141–70; Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society*, 1987.

[8] For critiques along these lines see Steven Shapin, 'Talking History: Reflections on discourse analysis', *Isis*, **75**, 1984, 125–28 and also Simon Schaffer, 'The eighteenth Brumaire of Bruno Latour', *Studies in the History and Philosophy of Science*, **22**, 1991, 174–92.

processes. These structures need to be examined in any worthwhile account of scientific discovery. When Nickles complains that 'social critics' have reduced the topic of discovery to 'vanishing point', he should be taken as meaning that 'discovery' as traditionally understood is no longer examinable.[9] Barnes does at one point suggest that the term be removed from the lexicon: 'To speak of "discovery" is to abet a form of collective self-forgetfulness, harmless, even "functional", in science itself, but disastrous if the aim is to study science.'[10] However, the work of Brannigan and Barnes does not mean that discovery ceases to be interesting; attention merely shifts to understanding the processes of attribution. Structures of argument in the making of attributions by different groups, processes whereby discovery events are naturalized and discoverers reified, and historical variations in what Brannigan calls the 'grammar' of discovery, become the focus of attention.

One danger of a sociological approach to discovery lies in a lack of historical sensitivity in the face of the interests involved. The play of interests occurs within a cultural field that shapes and constrains the freedom of interested parties to pursue their goals. In other words, structure exists as well as agency. Simon Schaffer's discussion of discovery accounts in the late eighteenth and early nineteenth centuries makes some important points in this regard. During that period the view of heroic authors of scientific discovery was in the ascendant. Lengthy historical processes that produced objects labelled discoveries were rewritten to represent discovery as an 'individual moment with an individual author'.[11] Whilst, importantly, these discovery stories were designed to support particular sets of technical practices associated with the nominated discoverers, they also served more general ideological functions. Among those functions was the legitimation of a more specialized research community defined against the more open forums of eighteenth-century natural philosophy. The issue of who could legitimately participate in scientific discourse was being negotiated. So was the closely related issue of who could legitimately write the history of scientific discovery.

Deploying the Attributional Model: The Case of the Water Controversy

The general claim made by advocates of the attributional model of discovery is that discoveries are not to be found. They are not to be found in the heads of individuals or in the state of preparedness of a culture. I would claim, however, contra the discourse analysts and the actor-network theorists, that discoveries can be found in a culture being more or less successfully attributed to particular individuals.

An attributional study of the water controversy involves, therefore, a number of elements. First it involves identifying the texts in which attributions are made. Second we must clarify the tactics and strategies used by those making the attributions. Third we seek to explain why the varied attributions were made and why and how particular tactics and strategies were used in trying to make them

[9] Nickles, 'Discovery', p. 164.

[10] Barnes, *T.S. Kuhn and Social Science*, p. 45.

[11] Simon Schaffer, 'Scientific discoveries and the end of natural philosophy', *Social Studies of Science*, **16**, 1986, 397.

stick. My basic assumption is that attributions are 'interested'. They are made as they are because there is some perceived advantage to be gained in so doing. The interests concerned may be of a technical kind. As Schaffer puts it, arguing for a particular discovery account may be driven by the concern to 'fix' certain technical practices. At other times wider interests may be involved in attributions of discovery. The wider issues might be more general questions of scientific style in which individual discoverers come to symbolize the disciplinary enterprise or, indeed, science as a whole but with particular institutional characteristics. Ultimately, then, attributions can be understood by referring them to causes in the cultural context. By approaching them in this way we learn about the process of discovery itself and we also learn about the culture whose attributions we are studying – in the case of the water controversy, early Victorian scientific culture.

I now want to distinguish major strategies of attribution in the nineteenth-century water controversy and also to indicate a range of forums within which attributional processes took place. I then go on to suggest how we can relate these strategies and forums to the characteristics of the opposing groups.

Strategies of Attribution

Let me deal with strategies first. Gross has suggested that there are a number of basic argumentative strategies available in a situation where priority to a discovery is contested.[12] In following one strategy, supporters of a candidate-discoverer challenge the 'sameness' of the respective discoveries of the two, or more, candidates. It can be argued that the apparent sameness of the discovery claims is illusory. The favoured candidate is depicted as putting forward a genuine, correct, or 'the best' knowledge claim whilst the others are painted as being wide of the mark in some way. If such a case can be argued convincingly, then questions of priority, in a temporal sense, become irrelevant. We might call this the 'argument from difference'. The fact that someone comes up with the wrong finding before your candidate signifies nothing if your candidate has the right one. A second approach is to assert the identity or 'essential similarity' of the discovery statements of the rival candidates, to say that they had the same idea, but to argue that one of them had temporal priority. We will call this the 'argument from synonymity and priority'. A third approach is to contend over matters of 'right method'. In fact this might, depending on circumstances, be treated as a separate approach or seen as an aspect of the 'sameness' or 'difference' of competing candidate discoveries.[13]

I will demonstrate that the Watt/Cavendish case did involve a cleavage along just these lines. Generally, Watt's supporters contended that the claims to discovery put forward by Watt and Cavendish were essentially the same. Their man deserved credit, they argued, by virtue of his priority. He had *the* idea (singular) first. The contest becomes a circumstantial one. *What* was claimed was not at issue but *when*

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[12] Alan G. Gross, 'Do disputes over priority tell us anything about science?', *Science in Context*, **11**, 1998, 169–70.

[13] On the uses of method see John Schuster and Richard Yeo (eds), *The Politics and Rhetoric of Scientific Method*, 1986.

it was said, or stated, was argued about. Watt's camp, then, wanted to play the game of circumstantial evidence. The Cavendish camp would also play that game up to a point. But they did not regard it as their strongest suit. Rather, they tried to show that questions of timing were otiose because the supposed discovery that Watt announced was in fact a farrago of nonsense. Watt's announcement may have been first, but it was just plain wrong, it lay outside the line of development of the New Chemistry and therefore it could not be identified as a discovery.

Let me give some examples to illustrate these strategic and tactical variations by examining briefly how claims were made for the essential similarity or difference of the discovery statements of Cavendish and Watt.[14] This clearly involved definition of what constituted the discovery. Here are some statements on this issue from key texts.

James Patrick Muirhead asked the question this way:

> [W]ho first explained the real cause of the formation of the water, by drawing and stating the conclusion that water is composed of two gases, which unite in the process of their combustion, or explosion … Who was in point of fact the first to make public that theory, after having formed it altogether independently of the idea of others[?][15]

Muirhead's framing of the question in this way assumed and sought to enforce the essential similarity of Watt's and Cavendish's statements. What we are directed to look for are statements saying that 'water is composed of two gases …'. At this level, Watt and Cavendish can be seen as stating the same thing even though they apparently differed on what those gases were. Muirhead is claiming that their statements are similar *enough* to be treated as identical. Muirhead is playing down the importance of the issue of the exact identity of the gases, a point that his opponents regarded as much more important. Indeed one of the grounds used by those arguing for Cavendish was that Watt laboured under misapprehensions about the nature of the gases involved. Making that point, or indeed arguing against it, involved entering quite deeply into the chemistry, a route that Muirhead wanted to avoid. If Muirhead could gain acceptance of his account of what the essence of the discovery was, then he could concentrate on the circumstantial evidence concerning *when* the two candidate discoverers first made their ideas public. He argued that Cavendish did not provide conclusions from his experiments until July 1784 whereas Watt had made his idea known much sooner. As Muirhead put it at one point, 'the question has become one of evidence much more than chemistry'.[16] The question did not just 'become' one of evidence. It was a matter of conscious and strenuous effort by Muirhead and his allies to portray the question in that way and to keep the controversy focused on that approach.

A second kind of statement came from William Vernon Harcourt and William Whewell. In the published version of his Presidential Address to the British

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[14] These examples involve me in making statements out of context. I do this only in order to illustrate my point concretely. The statements are thoroughly contextualized in later sections of the book.

[15] J.P. Muirhead (ed.), *The Correspondence of the late James Watt on his Discovery of the Theory of the Composition of Water*, 1846, p. xxxiv.

[16] Muirhead (ed.), *Correspondence*, pp. cxvii–cxviii.

Association for the Advancement of Science at Birmingham in 1839, Harcourt stated:

> Whilst the views of Cavendish are shown by the internal evidence of the experiments themselves … to have been from the first precise and philosophical, those of … Watt were … vague and wavering to a degree scarcely comprehensible to those who have not studied the ideas prevalent at that period of chemical history.[17]

According to Harcourt, then, far from the views of Watt and Cavendish being essentially similar, as Muirhead claimed, they were completely different. Watt's statement was 'vague and wavering', Cavendish's was 'precise and philosophical'. This was partly to do with Watt's ideas about what the gases were, but it was more to do with the claimed inadequacies of his conception of the role of heat in the composition of water and his failure to quantify. Whewell put this clearly in a private letter of support written to Harcourt:

> Your case, as you first put it, remains to my mind quite unshaken. But perhaps you may be wishing to know how the subject presents itself to my mind, looking at it rather with reference to the general course of the history of Science than to any special evidence of dates and the like …
>
> I think your remark is quite decisive – that Watt's views are utterly damaged by involving a composition of ponderable and imponderable elements. This of itself was enough to show that he did not consider elementary composition with the rigour and distinctness which the discovery of the synthesis and analysis of bodies at that time required … [Watt's hypothesis] was by its very terms unsuited to the step which science then had to take.[18]

This last statement by Whewell quite explicitly placed Watt outside what he considered to be the line of progress in science. As we will see, Whewell and other members of the Cavendish camp had a quite exclusive self-conception as arbiters of the history of science. Because this statement was made in a private letter, its attributional significance is much reduced (its persuasiveness was directed at Harcourt only and he was already converted!). Of more significance was what Whewell had to say in his *History of the Inductive Sciences*. In the second edition Whewell gave the credit to Cavendish and made no mention of Watt at all. In the third edition he repudiated 'recent attempts to deprive Cavendish of the credit of his discovery of the composition of water, and to transfer it to Watt …Watt not only did not anticipate, but did not fully appreciate the discovery of Cavendish and Lavoisier …'.[19]

Let me give a final example of how members of the Cavendish camp sought to open up chemical differences, that is, to stress the differences between the discovery statements of Watt and Cavendish. This comes from George Wilson, the author of *The Life of the Hon*^ble^ *Henry Cavendish*, published in 1851. This is a severe and

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17 William Vernon Harcourt, 'Address', *Report of the Ninth Meeting of the British Association for the Advancement of Science held at Birmingham in August 1839*, 1840, p. 23.

18 Whewell to Harcourt, 11 February 1840, in *The Harcourt Papers*, vol. 14, pp. 105–106.

19 William Whewell, *History of the Inductive Sciences, From the Earliest to the Present Time*, third edn, 1857, vol. 3, pp. 111–12.

complex book. Wilson himself, quite a witty man, once described it as a 'very dry book despite the amount of water in it'. Most of it consists of a study of the water controversy. It is also a paradoxical work combining as it does some very sophisticated historical analysis and some very crude presentist historiography.

In the same vein as Harcourt but in much more detail, Wilson claimed to show that Priestley's experiments, upon which Watt relied in drawing his conclusions, used charcoal in preparing his inflammable air. This could not have produced the pure mixture of gases in the correct ratio to leave only water on combustion. Therefore, in so far as Watt's inferences were correct, they *could not have been properly drawn* from Priestley's experiments. Thus either we say that Watt drew his inferences from Cavendish's experiments as imperfectly transmitted via Priestley's attempt to replicate them, or we cut Watt's inferences adrift from a sound experimental basis altogether and they become groundless, if inspired, speculations. Either way, the idea that Watt inferred a theory of the composition of water by a sound method is made to look suspect and Cavendish is left as the only candidate in the field.

Wilson believed that the issue of discovery could and should be decided on his knowledge of the true state of affairs. And that true state of affairs centred most clearly on the sustained experimental programme of Cavendish, his careful, judicious and accurate method of working:

> Had [Cavendish] never experimented, or had he never reported his results to Priestley, there is no reason to suppose that Watt would have conjectured, even remotely, that water is a compound of oxygen and inflammable air. *He was not on the track of such a discovery*. His speculations on the convertibility of steam into a permanent gas by the change of all its latent into sensible heat, did not point in that, but in exactly the opposite direction. He was following Priestley in all his devious wanderings, and going astray along with him … when Priestley's repetition of Cavendish's experiments (in which all that was true and significant was Cavendish's) arrested him in his mistaken course, and enabled him to approximate to the true theory of the composition of water.[20]

It is important here to recall Wilson's and Harcourt's overall conception of what the discovery of the composition of water consisted in and what its significance was. That significance was not confined to showing that water is a compound rather than an element. It involved the typification of a whole class of chemical reactions and therefore the establishment of the basis of modern chemical practice. On this conception, the discovery could only be properly found in the wider context of a disciplined and sustained programme of research of the kind, and the range, pursued by Cavendish and not so readily attributable to Watt. Wilson's whole account of the controversy was predicated on a concern to assert the standards of modern chemical discipline that Cavendish was taken to prefigure. 'Discovery' is treated within that disciplinary framework and in the end Cavendish and Watt are judged against it.

Here, then, are some examples of different attributional strategies. Throughout my analysis of the water controversy I will be concerned to identify and interpret the attributional strategies employed by different individuals and groupings. I will

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[20] George Wilson, *The Life of the Honourable Henry Cavendish*, 1851, p. 437. (My italics.)

also be concerned with the context of these strategies in relation to the forums in which they were deployed and the interests that they served. We can now examine the question of attributional forums.

Forums of Attribution

Attributional processes occur in a variety of forums. The most obvious place to look for attributions (and in many controversy studies the only place investigated) is in texts making explicit contributions to the 'controversy itself'. So, for example, François Arago's *Eloge de James Watt* is recognized by everybody as a central document in the water controversy. In his *Eloge* Arago attributed the discovery of the composition of water to James Watt. Depending upon how the *Eloge* was published, it would have a certain readership. (Initially published in French, it would have had a limited British readership if it had not been translated into English. In fact it was translated twice into English, published twice as a pamphlet and also in the *Edinburgh New Philosophical Journal*.) Accounts of such central publications in newspapers, in the weekly and monthly press, and in the reviews reached much wider audiences still. So did articles in encyclopaedias on 'chemistry', 'water', 'James Watt', 'Henry Cavendish' and so on. In these texts, too, statements were made about who discovered the compound nature of water. Generally, these statements were simpler than those in the central documents of the controversy. Other important sorts of texts were delivered lectures or circulated lecture notes and also chemistry textbooks. The latter might employ a historical method of presentation or they might lard an analytical approach with the odd bit of historical 'colour'. Such texts were presumably important in so far as they would help to shape the next generation of expert opinion. The problem with texts, of course, is that it is notoriously difficult, perhaps impossible, to determine what readers took away from them (if anything). That attributions of particular sorts were made can be established. How they related to opinion within various communities is very uncertain.

Any form of human communication is potentially a vehicle of attribution: inscriptions on monuments; conversations with 'informed' individuals. Conversations have, of course, disappeared into the ether and are not to be captured. Interestingly, Watt Jr did once suggest to Henry Brougham that he include reference in the inscription for a monument to Watt's discovery of the composition of water. Writing claims in stone may have much to recommend it, but in terms of potential audience paper is far more effective.

So forums of attribution are numerous. Are they all as important as each other? One distinction we might be tempted to make is that between forums for 'expert' opinion and those for 'popular' opinion. We tend to think that forums in which expert opinion is established are more important (in the sense of authoritative) than those in which popular opinion is formed. Controversy studies have often distinguished, following Harry Collins,[21] between a 'core set' and the rest. The

[21] Harry Collins, *Changing Order. Replication and Induction in Scientific Practice*, 1985, pp. 142–48.

'core set' is that grouping whose decisions on a matter tend to be decisive because they are recognized as the key authorities, as the best-informed judges of the science in question and so on. Perhaps it does not matter what readers of popular, ephemeral literature imbibed by way of attributions on such questions. In so far as participants in the controversy have influence over what happens in various forums, then they can, of course, seek to shape processes of attribution. However, any easy distinction between 'expert' and 'popular' forums will be avoided since the boundary line was under negotiation during the period we are concerned with. Such distinctions are not timeless nor are they uniquely specifiable. Where the line is to be drawn tends, in fact, to become part of the controversy.[22] In this study, for example, the status of encyclopaedias as expert or popular forums is varied, ambiguous and changing and the contending parties are engaged in a mutual questioning and claiming of expertise. It is in any case arguable that the more popular forums, with their practical demands for simplification and brevity, are in fact crucial to the naturalizing of discoveries and the reification of discoverers.

Strategy and Structure

The argumentative strategies and forums used by participants in controversies are not randomly distributed. There is usually a discernible relationship between the strategies adopted, the forums employed, the characteristics of the strategists, and the structural features of the situation that the groups find themselves in.

It has struck me, for example, that the Watt camp had certain collective characteristics that would predispose them towards the argument from circumstantial evidence to temporal priority (the argument of synonymity and priority). By the Watt camp I mean primarily James Watt Jr, Henry Brougham, Francis Jeffrey, James Patrick Muirhead and François Arago. I suggest that all these characters were 'outsiders' so far as British science, let alone chemistry, was concerned in the 1830s to 1850s when the controversy was at its height. I believe that the argument from circumstantial evidence for temporal priority was primarily an outsiders' argument in this case.

In thumbnail-sketch terms we can say that Jeffrey was a literary critic, known as the founder and long-term editor of the *Edinburgh Review*. He was also a prominent advocate and a judge. Before he became Watt Jr's eyes and hands, James Patrick Muirhead was an Oxford-trained lawyer with antiquarian and Scottish historical interests. Brougham at one time or another might have claimed scientific insider status: in the 1790s, while still a student at Edinburgh University, Brougham published work on optics in the *Philosophical Transactions of the Royal Society of London*. In his old age in the 1850s Brougham returned to optics as a supporter of Sir David Brewster's rearguard action for the corpuscular theory of light against the

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[22] See Stephen Hilgartner, 'The dominant view of popularization: Conceptual problems, political uses', *Social Studies of Science*, **20**, 1990, 519–39, also Anne Secord, 'Science in the pub: Artisan botanists in early nineteenth-century Lancashire', *History of Science*, **32**, 1994, 269–315 and Bernadette Bensaude-Vincent, 'A genealogy of the increasing gap between science and the public', *Public Understanding of Science*, **10**, 2001, 99–113.

undulatory orthodoxy of the scientific establishment within the Royal Society and the British Association. In between Brougham was a successful and celebrated lawyer, the defender of Queen Caroline, Lord Chancellor in Lord Grey's reform government, and a major problem for his own party, the Whigs. Watt Jr might have claimed some general scientific credentials during his youth, during his revolutionary days in France and also in connection with experiments on steam navigation. But he certainly did not claim such expertise by the 1830s and 1840s. Arago is more problematic for my argument in the sense that he was obviously a very prominent scientist as Perpetual Secretary of the Académie des Sciences. However, his reputation was primarily as an astronomer. And that reputation was not impeccable because his judgement was seen by many as compromised by nationalistic fervour, radical political views and a consequent belief in promoting the openness of scientific institutions to an inappropriate extent.

The case of Arago raises a general point about the scientific credentials of all these Watt supporters. Those credentials had to be negotiated in context. It would be *possible* to argue for the scientific understanding brought to their work by all of these characters. Even J.P. Muirhead at one point claimed to have a technical understanding of the chemistry involved in the water controversy second to none. Having read much of his correspondence and seen how, privately, he took on the arguments of some of the expert chemists on the Cavendish side, I would have to agree with him. Muirhead could have made claims to expertise and held his own in substantive chemical argument and interpretation, if he and his allies had chosen to do so. He did not so choose. Brougham at times *did* choose to claim expertise. His self-diagnosed omniscience was hard to control. His allies winced and feared for their cause when Brougham put his scientific, or worse his chemical, hat on. Arago's *prima facie* scientific credibility could be, and was, readily challenged by his opponents in the water controversy.

So, remembering always this point about the negotiated character of expertise, we can say, however, that generally speaking the Watt camp did not build their case on knowledge of chemistry (of the 1840s or the 1780s). They spent little time discussing the meanings, and the rival merits, of Watt's and Cavendish's discovery statements. Their professional expertise lay, if anywhere, in the law, in argument about evidence. Understandably, then, their focus was upon the circumstantial evidence of temporal priority. They insisted that the key issue at stake in the controversy was more akin to a legal than to a scientific one, and that the ability to judge such matters was not confined to scientific specialists. This was put plainly by Francis Jeffrey to George Wilson: 'I know I am but a child in your mind in any question of chemistry and I have nothing but my poor Logic to combat your Science with. But … I have taken up a strong impression that the question we have to consider is much more a question of logic than of science …'.[23] When Watt Jr selected Muirhead to undertake the editing of his father's correspondence on the water question, he expressed the view that 'As a question of evidence, this falls peculiarly within the sphere of your pursuits.'[24] In arguing their case on the basis of

[23] Francis Jeffrey to George Wilson, n.d. [1847], Special Collections, Edinburgh University Library Dk. 623/1/37.

[24] Muirhead (ed.), *Correspondence*, 1846, p. ii.

circumstantial evidence the Watt camp were confident that they could carry 'public' opinion with them. Muirhead and Watt Jr relied heavily on distributing free copies of the *Correspondence* when it was published in 1846. As responses came back from those receiving complimentary copies, Muirhead expressed confidence that they were winning the battle:

> The feeling has been so uniform in all the letters I have recd from unscientific readers that I am certain a very strong impression has been produced as to your father's priority. This is a great point, and may help to reconcile us to any rubs that may come from the doctissimi – Forbes and all his Company![25]

These 'unscientific' readers also often used legal analogies when writing about the priority dispute. Thus, Gilbert Hamilton wrote to Muirhead: 'I can only hold such sentiments as a Juror may draw from the evidence before him & I do believe no felon was ever hanged on surer testimony.'[26] The legitimacy of non-expert judgement was at stake here. If the discovery can be attributed on the basis of circumstantial evidence, then 'everyman' is a legitimate judge of the scientific priority dispute, just as 'everyman' is a legitimate juror under the law. Cavendish's supporters were to challenge all these interlinked positions.

We have seen something of the basic attributional strategy of the Cavendish camp in the brief extracts from the writings of Harcourt, Whewell and Wilson. Who were the members of this camp? They were members of the scientific élite of Oxford, Cambridge, London and Edinburgh. They were prominent among the leadership of the British Association and the 'rising stars' of the Royal Society of London. Besides Harcourt and Whewell we might mention George Peacock, prominent mathematical reformer, Trinity College man and Dean of Ely; George Airy, Astronomer Royal; James David Forbes, Professor of Natural Philosophy at the University of Edinburgh. These were joined by members of the chemical community such as Professor Thomas Graham. Graham was a Scot, trained under Thomas Thomson, who took the chair of Chemistry at University College London. In the 1840s Graham was President of the newly founded Chemical Society of London and also of the 'Cavendish Society', a chemical publishing society that published, among other works, Wilson's *Life of Cavendish*.

Although members of this grouping were collectively, if haltingly, approaching occupational professionalism, they were decisively status professionals. That is, they were making a strong bid to control the conduct of esoteric science as pursued by disciplined, trained research scientists. They were fighting at a number of boundaries: with advocates of 'fringe sciences' such as phrenology; with the 'social scientists' and statisticians; and, very significantly for us, with 'practical' scientists and engineers. These sorts of struggles were common in the nineteenth century. The Cavendish camp was engaged in what Tom Gieryn has called 'boundary work'.[27] Cavendish was a symbol of precise, disinterested, sustained

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[25] Muirhead to Watt Jr, 23 December 1846, Muirhead Papers, MS GEN 1354/1081.

[26] Gilbert Hamilton to J.P. Muirhead, 5 January 1847, Muirhead Papers, MS GEN 1354/161.

[27] See, for example, Thomas F. Gieryn, 'Boundary-work and the demarcation of science from non-science: Strains and interests in professional ideologies of scientists', *American Sociological Review*,

research, a symbol in the self-image of his advocates and supporters. Watt symbolized the autodidact, scientist-engineer, vital to the welfare of the country but not, so far as members of the Cavendish camp were concerned, to be confused with the 'real' scientists. The élite chemists saw their science as overshadowed by its reputation for coarse practicality and in need of academic respectability. Identification with the likes of Cavendish, and gaining the approval of the Cambridge men, were important aspects of that campaign for respectability. These are some of the aspects of professional vested interests that rendered the Cavendish/Watt issue important in early Victorian Britain and that will be explored further.

The control of interpretation of the history of science was also of vital importance to the Cavendish camp. Whewell's *History of the Inductive Sciences* was a key document in this control. It was not a personal project on Whewell's part but a communal one in which his inner circle advised him on the winnowings, selections, inclusions and exclusions that should in their view be made in the historical account, especially with regard to recent scientific work. There was an attempt to assert that only those in command of the science were in a position to arbitrate on matters of scientific discovery. The water controversy clearly involved the assertion of this principle, and it was reflected in the attributional strategy pursued by Whewell and others of the Cavendish camp, against the legalistic, everyman-his-own-judge, approach of Watt's supporters. The 1840s and 1850s were, I think, the key decades during which this ascendancy of expertise was secured. By the 1850s and 1860s not only the major societies and their publications were controlled by the pro-Cavendish élite, but also key vehicles of scientific popularization and historical memory. This control was not complete, however. Other literary areas were not so controlled and there we find for many years examples of attributions to Watt. We will see such examples in textbooks, popular encyclopaedias and in what might be called a nationalistic Scottish literature.

Earlier historians of the water controversy have tended to focus on resolving for themselves the question of who *really* discovered the composition of water. They have invariably adopted one perspective or another and come out in favour of the obvious candidate. If they have treated the matter as essentially one of circumstantial evidence, then they have followed the Watt camp. If they have engaged with the minutiae of the chemistry promulgated by Watt and Cavendish to distinguish between their conceptions and render the temporal dimension irrelevant, then, they have tended to find in Cavendish's favour, or perhaps in Lavoisier's. In my study I have sought to avoid this. The key to understanding the 'victory' of Cavendish lies not in what he really did in the 1780s, even presuming that that reality could be unearthed in some non-committal way. Rather the key lies in what was done by assorted participants in the second phase of the controversy in the 1830s through the 1850s and during subsequent textual attributions. 'Discovery' is a designation or status accomplished through attributional processes. It is not enough, as Schaffer has argued, to deconstruct heroic discovery stories. We need to understand the functions

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48, 1983, 781–95 on John Tyndall. In the United States, the case of Thomas Edison's relations with the scientific community is an instructive one. See David A. Hounshell, 'Edison and the pure science ideal in 19th-century America', *Science*, **207**, 8 February 1980, 612–16.

of such mythical accounts within the communities that produce them.[28] The task of the historian, in my view, is to try to understand and explain the variety of attributions made, the strategies employed in making them, and why and how some won out over others. We thus need to establish the competing versions of the idea of 'discovery' being promoted by the opposing sides, at a time when that idea was in the midst of a major cultural transition.

[28] Simon Schaffer, 'Making up Discovery', in Margaret Boden (ed.), *Dimensions of Creativity*, 1994, p. 18. In so far as myths serve functions for communities, the deconstruction of the myth of the heroic individual discoverer is often done in the name of collectivist views of the nature of science. For an interesting recent case study see Abigail O'Sullivan, 'Henry Dale's Nobel Prize Winning "Discovery"', *Minerva*, **39**, 2001, 409–24.

Chapter 3

The Beginnings of a Dispute and its Interpretation

Introduction

It is impossible to simply state the facts concerning the eighteenth-century research relevant to the discovery of the composition of water. However, some such attempt must be made so that various occurrences are made familiar for the purposes of interpretative discussion. As we will see, not the least of the artificialities in recounting research on the composition of water is that most of the work was directed at other objectives at the time. Only in retrospect did the various activities appear as a coherent, linear progress towards a discovery. Only in retrospect did that discovery come to have an important place in the larger transformation in thought and practice that came to be known as the Chemical Revolution.

Having outlined the major events leading to the 'discovery' and also the first-phase controversy over that discovery (in so far as there was one in the late eighteenth century), we will then sketch the historiography of the Chemical Revolution. Depending upon the historiographical position that is adopted concerning that larger transformation, the character and significance of the discovery of the compound nature of water changes. We will also pursue a deeper contextual account of the work of both James Watt and of Henry Cavendish on water. This is in order to show how far the conceptions of each investigator departed from those subsequently, and most commonly, attributed to them by various parties within the historiography of the Chemical Revolution. Once this is accomplished, we are well placed to pursue the arguments and attributional strategies of the succeeding decades.

The Story of Water

In recounting the story of water in the 1780s my aim is to set out the key events and processes that, by common consent of nineteenth- and twentieth-century historians, constituted the basic framework of investigations into the nature of water. Once again, it is not my intention to try to resolve the controversy in this, its first, phase. Nor is it my intention to tell the fullest story possible. For that, Partington remains an indispensable source.[1]

The story begins conventionally with experiments by various investigators that involved the explosion of inflammable air with either common air or dephlogisticated

[1] J.R. Partington, *A History of Chemistry*, vol. 3, 1962, pp. 325–62, 436–57.

air. Joseph Priestley was among the first when, in 1775, he noted that the explosion was much louder when dephlogisticated air was used than when common air was employed. The scientific interest at this point lay in the strength of gaseous explosions. Volta introduced another dimension to such experiments when, in 1776–1777, he developed the technique of firing gaseous mixtures in closed vessels using an electric spark to ignite them. Volta invented a eudiometer to prosecute such experiments and to measure the volume changes of the different airs when such explosions occurred. The itinerant lecturer in natural philosophy, John Warltire, a friend and correspondent of Joseph Priestley, was also firing explosive mixtures. It appears from his correspondence with Priestley that they conducted firings in glass vessels and observed the deposition of moisture on the inside of the vessel, though this was mentioned only with regard to explosions of inflammable and common air. Priestley's account of this in his *Experiments and Observations*[2] recounts Warltire's belief that the appearance of the moisture confirmed a long-held view that common air, when phlogisticated, 'deposits its moisture'. The belief was that the moisture was present in the gaseous mixture and deposited (not produced) by the explosion. This observation, though clearly of interest, was made almost in passing. Priestley returned immediately to the issues of the violence of the explosion and the unfavourable comparison between the firing of inflammable air and of gunpowder. Warltire apparently conducted these experiments with a view to determining the change in weight after the explosion. He claimed to find a loss of weight.

Henry Cavendish began a celebrated series of experiments in 1781 upon which his claims to the discovery were to be based. These, it was contended, were the experiments eventually published as 'Experiments on Air' in the *Philosophical Transactions* in 1784 and 1785. Cavendish's announced aim was to study the 'phlogistication' of common air, a process well known to diminish the volume of the air.[3] He was concerned to find out what became of the air that was 'lost or condensed'. Thus phlogistication by explosion with inflammable air was only one of a number of processes that Cavendish was concerned with. (The others were calcination of metals, the burning of sulphur and phosphorus, the mixture of common air with nitrous air, respiration of animals, and the action of an electric spark.)

When it came to the explosion with inflammable air, Cavendish acknowledged Warltire's experiment but, unlike him, did not record a significant loss of weight. Cavendish was still guided in his measurements by his eudiometric experiments published earlier, in 1783. That is, he was concerned with the diminution of the common air by phlogistication. He measured the point at which complete phlogistication (or maximum diminution) occurred as being 423 measures of inflammable air to 1000 measures of common air. When a mixture with these proportions was exploded, 'almost all the inflammable air, and about one-fifth of the common air, lose their elasticity, and are condensed into the dew which lines the glass'.[4] Cavendish then devised an apparatus to burn together larger quantities of inflammable air and common air in order to obtain a more substantial quantity of

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[2] Joseph Priestley, *Experiments and Observations*, vol. 2, 1781, pp. 395–98.

[3] Henry Cavendish, 'Experiments on air', *Philosophical Transactions*, **74**, 1784, 119–53.

[4] Ibid., p. 128.

the 'dew'. By various tests he found the dew to be plain water. The phrase he used was that the inflammable air and about one-fifth of the common air 'are turned into pure water'. Our natural assumption would be that Cavendish meant that the gases had formed water as a compound. However, Cavendish did not say that, and, as we will see upon examining this point in more detail, there are good reasons for thinking that at this stage Cavendish's explanation for what was going on here invoked a condensation, not a compounding process.

Cavendish's next series of experiments involved exploding various mixtures of inflammable and dephlogisticated airs. He found that when mixed in the proportion very close to 2 to 1, these airs were converted into the same weight of water; that is, the weight of the airs before the explosion was equal to the weight of water present after it. However, and this too was of great consequence, Cavendish found the liquid to be acid to the taste and identified a small quantity of 'nitrous acid' in the water. He eventually explained this fact in the paper published in 1785.[5]

It is important to remember that, although Cavendish conducted these experiments from 1781, they were not published until 1784. When Cavendish commenced his experiments, Watt was returning to some of his earlier investigations with steam. He was in regular contact with Joseph Priestley, Joseph Black and others on chemical matters. As we will see in more detail later, Watt had formed a view about the possible conversion of water into air if it were heated. In late 1782 Priestley reported an experiment using an earthenware retort in which it seemed that precisely this did happen; that is, water was converted into air. The experiment was, however, subsequently explained in another way and the effect discredited. This did not undermine Watt's faith in his theory. In early 1783 Priestley learned of Cavendish's experiment in which dephlogisticated air and inflammable air were exploded by the electric spark, producing a deposit of water of equal weight. Priestley determined to repeat this experiment, which he claimed to have done. He was not entirely happy with his weighings but stated that he 'always found, as nearly as I could judge, the weight of the decomposed air in the moisture acquired by the paper'.[6]

Much was to hang on this experiment during the second phase of the water controversy. The reason is as follows. Priestley's ostensible repetition of Cavendish's experiment was subsequently judged not to be so in a crucial respect. In order to make sure that his inflammable air was dry (and thus not contaminated with the very substance whose production was at issue) Priestley produced it by heating charcoal in an earthenware retort. For the same reason, he also produced his dephlogisticated air by heating nitre. Priestley regarded these measures as improvements upon Cavendish's experiments but not interfering with the essential similarity of the two sets of experiments. From the vantage point of post-Lavoisian chemistry the mixture of gases so generated (hydrogen, carbon monoxide, oxygen and nitrogen) could not physically have produced the same result as Cavendish's experiment did. Whether his ostensible reliance on Priestley's retrospectively flawed experiment fatally wounded Watt's claim to discovery, or was irrelevant to it, was to be a matter of heated debate, becoming known as 'the charcoal argument'.

[5] Cavendish, 'Experiments on air', *Philosophical Transactions*, **75**, 1785, 372–84.

[6] *Philosophical Transactions*, **73**, 1783, 398–434, at p. 414.

For now, we need simply to note that, on learning from Priestley of this experiment (or via Priestley of Cavendish's experiment) Watt was moved to venture the hypothesis that 'water is composed of dephlogisticated and inflammable air, or phlogiston, deprived of part of their latent heat'. This view, expressed in a letter to Joseph Black of 21 April 1783, was repeated in much this form in letters to Gilbert Hamilton and J.A. De Luc at about the same time.[7] Watt set out to formally communicate his ideas by writing a letter to Priestley on 26 April 1783 that was intended to be read to the Royal Society of London, along with Priestley's account of his experiments. However, on 29 April Priestley informed Watt about the problem with the earthenware retort experiments, in which water was ostensibly converted into air. Watt decided to withdraw his letter and asked that it not be communicated to the Royal Society at that point. The letter had been sent to London, however, and was seen by a number of people in scientific circles there.

As Watt's communication lay in abeyance, other developments were in train involving Lavoisier. On 24 June 1783, Lavoisier and Laplace performed an experiment burning inflammable and dephlogisticated air in a closed glass vessel and obtained a quantity of water. This was a rather hasty effort and not characterized by the careful quantification usually associated with the work of these natural philosophers. A brief account of this experiment was published in December 1783.[8] Quickly and efficiently, Lavoisier and his collaborators went on to devise and prosecute quantitative experiments culminating in the famous and masterly experiment in which water was decomposed and then synthesized.[9]

Stimulated by Lavoisier's rapid moves, Cavendish had his first paper recounting his experiments on air read at the Royal Society on 15 January 1784. It was at this juncture that seeds of conflict between Watt and Cavendish were sown. Watt was preoccupied with steam engine troubles but on 1 March 1784 his friend De Luc, having obtained a copy of Cavendish's paper, wrote to Watt that: 'In short, he [Cavendish] expounds and proves your system, word for word, and says nothing of you.'[10] De Luc encouraged Watt (as Sir Joseph Banks had done previously) to submit to the Royal Society his original letter to Priestley and a new one written to De Luc which would then be read to a meeting of the Society. De Luc also advised Watt to be cautious and not cause jealousy that might jeopardize his fortunes with steam projects. Watt's reply to De Luc was a peculiar mixture of defiance and diffidence and is worth quoting at length:

> On the slight glance I have been able to give your extract of the paper [Cavendish's], I think his theory very different from mine; which of the two is right I cannot say; his is more likely to be so, as he has made many more experiments, and, consequently has more facts to argue upon.

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7 Watt to Black, 21 April 1783, printed in E. Robinson and D. McKie (eds), *Partners in Science*, 1970, pp. 124–27, at p. 126. For the letters to Hamilton and De Luc see J.P. Muirhead (ed.), *Correspondence*, 1846, pp. 20–21.

8 *Obs. Phys.*, **23**, 1783, 452–55.

9 For accounts of this see: Henry Guerlac, 'Chemistry as a branch of physics: Laplace's collaboration with Lavoisier', *Historical Studies in the Physical Sciences*, **7**, 1976, 205–16; Robert J. Morris, 'Lavoisier and the caloric theory', *The British Journal for the History of Science*, **6**, 1972, 1–38.

10 Muirhead (ed.), *Correspondence*, pp. 43–44.

> I by no means wish to make any illiberal attack on Mr. C. It is barely possible he may have heard nothing of my theory; but as the Frenchman said when he found a man in bed with his wife, 'I suspect something'.
>
> As to what you say of making myself 'des jaloux', that idea would weigh little; for, were I convinced I had had foul play, if I did not assert my right, it would either be from a contempt of the modicum of reputation which could result from such a theory; from the conviction in my own mind that I was their superior; or from an indolence, that makes it easier to me to bear wrongs, than to seek redress. In point of interest, in so far as connected with money, that would be no bar; for, though I am dependent on the favour of the public, I am not on Mr. C. or his friends; and could despise the united power of the illustrious house of Cavendish, as Mr Fox calls them.[11]

Watt resolved to have his letters on water read at the Royal Society. In London the following week he saw Sir Joseph Banks and, with De Luc's assistance, resubmitted his original letter to Priestley and a new letter to De Luc. Watt explained to Banks his reasons for withdrawing the paper originally. Watt was anxious that Banks not think 'his own [Banks's] honour a little called into question', nor did Watt want Banks to feel, as he evidently did, that the withdrawal was a slight upon the Royal Society.[12] This was a time when 'dissensions' racked the Society, and in this politically charged atmosphere significance was easily read into actions. One dimension of that dispute concerned Banks's supposed mistreatment of certain classes of person, especially provincial dissenters. On the occasion of one rejection of such a character for the Fellowship, Joseph Priestley threatened no longer to publish his writings through the Royal Society. It appears that Banks may have thought that Watt, who after all was closely associated with Priestley, had withdrawn his paper as a political act. McCormmach has suggested that the reading of Cavendish's 'Experiments on Air' on 15 January 1784 was a 'power move'. The previous three meetings of the Society had been disrupted by political debate and criticism of Banks and his conduct of the institution. Among the charges made by the opposition was that under Banks the Royal Society had become scientifically feeble compared with Newton's day. The reading of Cavendish's highly significant 'Experiments on Air' would have been a very useful retort to such claims.[13] The fact that Cavendish was one of Banks's most valued advisers and supporters at this time also fits the picture, as does the fact that when Paul Maty, one of Banks's chief critics, resigned the post of Secretary to the Royal Society, Charles Blagden assumed the position. Blagden was also Cavendish's assistant, and his behaviour in the water controversy was to be a major issue, as we shall see in a moment.

Further evidence that the water question became entangled at least a little with the wider politics of the Royal Society is provided by the fact that Maty, as editor of *A New Review*, ensured that only the part volume of the *Philosophical Transactions* containing Watt's paper (part 2) was reviewed in his journal. The other part volume

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11 Watt to De Luc, 6 March 1784, in Muirhead (ed.), *Correspondence*, 1846, pp. 47–49.

12 Watt to De Luc, 12 April 1784 in Muirhead (ed.), *Correspondence*, 1846, p. 51.

13 Russell McCormmach, 'Henry Cavendish on the Proper Method of Rectifying Abuses', in Elizabeth Garber (ed.), *Beyond History of Science. Essays in Honor of Robert E. Schofield*, 1990, p. 43. On the dissensions see also John L. Heilbron, 'A mathematicians' mutiny with morals', in Paul Horwich (ed.), *World Changes: Thomas Kuhn and the nature of science*, 1993, pp. 81–129.

containing Cavendish's paper was studiously ignored.[14] Although Watt was personally unwilling to become a *cause célèbre* in the Royal Society dissensions, others would have been happy to make him so. For later writers on the water question, this institutional dimension remained open as an issue to pursue. In the 1840s David Brewster was to seize it.

Returning now to Watt in the process of resubmitting his paper, we find him anxious to avoid suspicion that he had been making a political point, since Banks had 'always behaved in a friendly manner towards us'.[15] Characteristically Watt adopted a deferential manner, asking, among other things, that the Society excuse the defects of his style, 'which must naturally be concluded to savour more of the mechanic than of the philosopher'.[16] Watt's paper was read to the Society in late April and early May, spanning more than one meeting. Banks courteously reported the approbation it had received. Watt's deference masked resentments that spilled over when he wrote to friends outside official Royal Society circles. To Mr Fry in Bristol Watt reported that his papers had been read, and he promised to send copies if they were printed.

> But I have had the honour, like other great men, to have had my ideas pirated. Soon after I wrote my first paper on the subject, Dr Blagden explained my theory to M. Lavoisier at Paris; and soon after that, M. Lavoisier invented it himself and read a paper on the subject to the Royal Academy of Sciences. Since that, Mr Cavendish has read a paper to the Royal Society on the same idea, without making the least mention of me. The one is a French Financier; and the other a member of the illustrious house of Cavendish, worth above £100,000, and does not spend £1000 per year. Rich men may do mean actions. May you and I always persevere in our integrity, and despise such doings.[17]

The Royal Society's Committee of Papers ordered the printing of Watt's paper and Watt then negotiated with Blagden exactly how the two letters (to Priestley and to De Luc) would be published. Watt, like Cavendish earlier, made changes along the way.[18] A device was settled upon that avoided repetition but made clear the early date at which Watt had developed his key ideas.[19] By this time, however, the hope that his paper might appear in the same half-volume as Cavendish's 'Experiments

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14 See *A New Review*, **7**, 1785, 106–10. The review, almost certainly by Maty himself, strenuously defended Watt's 'hypothetical' contribution in a way that suggests that the hypothetical character of Watt's paper was a matter for comment and criticism at the time.

15 Watt to De Luc, 12 April 1784 in Muirhead (ed.), *Correspondence*, 1846, p. 51. The 'us' here is probably Boulton and Watt.

16 Watt to Banks, 12 April 1784, in ibid., p. 53. Such diffidence invited the response 'no, you are a philosopher'! As I have argued, it was important to Watt to be thought of as such. See David Philip Miller, '"Puffing Jamie"', *History of Science*, **38**, 2000, 5–7.

17 Watt to Mr Fry, 15 May 1784, in Muirhead (ed.), *Correspondence*, 1846, p. 61. As Muirhead notes, Watt probably meant to indicate Cavendish's worth as £1,000,000, which would have been more accurate.

18 The changes made in Cavendish's paper, including interpolations by Blagden, were to become part of the controversy and charge of conspiracy and fraud.

19 See Blagden to Watt and Watt to Blagden, 27 May 1784 in Muirhead (ed.), *Correspondence*, 1846, pp. 62–65.

on Air' was forlorn. It ended up being published in the subsequent half-volume. Beyond this the controversy between Cavendish and Watt seems to have created little stir. Watt certainly remained suspicious of what had gone on, especially with regard to various misdatings of documents, but if we are to believe the recollections of his son, no lasting antagonism existed towards Cavendish. When Watt was elected a Fellow of the Royal Society in 1785 he met Cavendish on friendly terms, and they remained on such terms until Cavendish's death.[20]

Watt took little or no part in the extended public debates between the supporters of phlogiston and the antiphlogistians led by Lavoisier. As Lavoisier and his colleagues realized the importance of the water question to the completion of the new chemistry of combustion, calcination and respiration, they performed more demonstrative experiments. Whether or not we as historians regard these moves by Lavoisier as the true clinching of the real composition of water or as an exercise in engrossment of the discoveries of others, they did generate the latter sorts of concerns in Britain. Blagden had visited Paris in June 1783. He was later to claim in a letter that was published in Crell's *Chemische Annalen* in 1786 that the experiment of 24 June, at which he was present, had been performed some days after he (Blagden) had informed Lavoisier and others of the work of Cavendish and Watt.[21] Efforts were made in Britain to resist not only the substantive case of the antiphlogistians but also the tendency of Lavoisier and his followers to gather more and more credit for the transformation of chemistry. However, British chemists, one by one, with the enduring exception of Joseph Priestley, acceded to the abandonment of phlogiston. Even as they did this, there was widespread uneasiness about the totalizing quality of Lavoisier's new chemistry, its new language and its seamless systemic quality. The adoption of the Lavoisian system remained equivocal. We will see in Chapter 4 that this equivocation allowed the survival for a time of what we might call 'hybrid' characterizations of the synthesis of water, hybrid in the sense that they often involved the unconscious marrying of older ideas with a Lavoisian gloss.

For the moment enough has been given of the story of water in the 1780s to gain our bearings in the second phase of the water controversy. Of course, getting those bearings also requires us to come to terms with the wider chemical context within which the water story was played out. For modern historians, and for the protagonists in the controversy in the mid-nineteenth century, the story of water was part of a major scientific transformation, a Chemical Revolution. It is to interpretations of that Revolution that we now turn.

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[20] In 1786, for example, Watt discussed with De Luc the misdatings of the preprints that Cavendish circulated and of his own letter to De Luc in the *Philosophical Transactions*. The former stated that Cavendish's paper was read in January 1783 (rather than 1784) and the latter was dated in April 1784 (rather than 1783). On Watt's relations with Cavendish see 'Letter from James Watt Esq. To the Editor', in Muirhead (ed.), *Correspondence*, p. iv.

[21] *Chemische Annalen*, 1786, 58–61. A translation of this letter is printed in Muirhead (ed.), *Correspondence*, pp. 71–74. For a rejection of such charges see Berthelot, *La Révolution Chimique, Lavoisier*, 1902 and for a gentle prosecution of them see T.E. Thorpe, 'Presidential Address, Section B', *Report of the Sixtieth Meeting of the British Association*, 1891, 761–71.

The 'Chemical Revolution' and its Historiography

The discovery of the composition of water cannot be addressed without coming to terms with the history of the 'Chemical Revolution'. Indeed, the understanding of the meaning of that discovery, its significance and even its existence as an event, depend upon what position one takes in the historiography of the Chemical Revolution. As I have tried to stress, my subject-matter is not the discovery of the composition of water 'itself' but rather nineteenth-century debates in Britain about that discovery. (Of course, if the attributional model of discovery is taken seriously, then the distinction between the discovery 'itself' and later debates about it is purely chronological rather than substantial.) It might be said that my concern is not with the history of the Chemical Revolution but with the history of ideas about that Revolution, or, put another way, with the history of histories of the Chemical Revolution. The disputants in the nineteenth-century water controversy were, effectively, arguing about the history of the Chemical Revolution and about the place of particular individuals within it. One way of reading this book (though frankly not the major intention in writing it) is as a contribution to the history of the concept of a late-eighteenth-century revolution in chemistry. That concept has had a career, and the origins and development of that career can be a matter of study in their own right.

John McEvoy and others have examined the career of the concept of the 'Chemical Revolution'.[22] McEvoy delineates a number of phases in the historiography in concentrating on the differences between what he calls the 'positivist-whig' interpretation on the one hand, and the 'postpositivist' on the other.

The 'positivist-whig' historiography, which McEvoy sees as dominating from the mid-nineteenth to the mid-twentieth century, was manifest in the well-known writings of historians such as Herbert Butterfield, Charles C. Gillispie, Robert Multhauf, James R. Partington, Douglas McKie and Maurice Crosland. This historiography has a number of features. It is progressivist in that it sees the Chemical Revolution as an episode in the emergence of a scientific approach to nature from a pre-scientific age in which phlogiston theory dominated. It is objectivist, though McEvoy does not use this term. Whether the objectivism was based in inductivism, falsificationism or conventionalism, the triumph of oxygen theory over phlogiston theory was regarded as sealed by the method of science. It is Manichean, treating the Chemical Revolution as a story of the victory of the forces

[22] John G. McEvoy, 'Positivism, Whiggism, and the Chemical Revolution: A study in the historiography of chemistry', *History of Science*, **35**, 1997, 1–33. See also John G. McEvoy, 'The Chemical Revolution in context', *The Eighteenth Century: Theory and Interpretation*, **33**, 1992, 198–216; Rachel Laudan, 'Histories of the sciences and their uses: A review to 1913', *History of Science*, **31**, 1993, 1–34; Bernadette Bensaude-Vincent, 'A founder myth in the history of science? – The Lavoisier case', in Loren Graham, Wolf Lepenies and Peter Weingart (eds), *Functions and Uses of Disciplinary Histories*, 1983, pp. 53–78; idem, *Lavoisier: Mémoires d'une révolution*, 1993, pp. 343–418; Arthur Donovan, 'The Chemical Revolution revisited', in Stephen H. Cutliffe, *Science and Technology in the Eighteenth Century: Essays of the Lawrence Henry Gipson Institute for Eighteenth Century Studies*, 1984, pp. 1–15. Also see the essays in Arthur Donovan (ed.), *The Chemical Revolution: Essays in Reinterpretation*, in *Osiris*, **4**, 1988, 5–231.

of light over those of darkness. In this connection the contest was usually set up as one between Lavoisier and Priestley, the former as the 'genius' in touch with the truths of nature, the latter as diverted from the true scientific path by his character and his religious and political obsessions. The positivist-whig historiography depicted scientific change as involving 'cognitive inversion' and as proceeding via 'crucial experiments' (of which Lavoisier's analysis and synthesis of water was one) and via 'eureka moments' of discovery. Another feature of this historiography was its emphasis upon the quantitative character of the new chemistry as a distinguishing feature of its scientificity when compared with the qualitative approach of phlogiston theorists. A strong founder myth pervades the literature of this historiographical tradition, especially, but not exclusively, among French scholars, as Lavoisier was painted as the founding father of modern chemistry. A variant on the founder myth, and one of much importance in our story, is that which placed Lavoisier under the guiding shadow of Newton. According to this 'Anglo-Saxon' view, the history of modern chemistry was to be found in a lineage that incorporated Joseph Black, Henry Cavendish and Lavoisier as well as Boyle, Newton and Dalton. Indeed, some chemist–historians of the later nineteenth century in Britain were to argue for Cavendish as the founder of modern chemistry. Some modern historians of science and the participants in the nineteenth-century water controversy shared the positivist approach.

The postpositivist approach has involved a challenge to the various features of the positivist historiography. Black and white became shades of grey. Scientific virtues were found in phlogiston theory in that it played a productive role in the development of pneumatic chemistry, the chemistry of airs. There was no necessary antipathy between chemistry pursued via phlogiston theory and a quantitative approach. Generally, the phlogiston theory and its associated practices were granted much greater integrity as a viable system of thought. This was a way of thinking about scientific paradigms that many of us learned from Kuhn. Lavoisier did not loom so large in this postpostivist historiography. The historical reputation of others was resuscitated, notably, of course, that of Priestley: 'Priestley functioned in the Chemical Revolution not as a dogmatic and entrenched defender of the phlogiston theory but as a source of much-needed criticism of Lavoisier and his supporters.'[23] The stark contrast between the rational, clear-sighted Lavoisier and his confused, benighted opponents is muted at the least. Along with epistemological views that problematized objectivism and sought to contextualize scientific change came the notion that Priestley's theological and political views and activities should no longer be seen as leading him astray scientifically. Rather they should be seen as shaping what was regarded increasingly as a legitimate set of scientific commitments and beliefs. The suspicion within this approach of the notion of 'crucial experiment' and the contention that experiments could only be crucial relative to a particular theoretical framework complicated the process of conceptual change. (This may explain, incidentally, the otherwise surprising lack of attention by historians in recent decades to the discovery of the composition of water.) As the Manichean conception of the Chemical Revolution has been superseded by an ecumenical one,

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23 McEvoy, 'Positivism, Whiggism', p. 23.

the urge among historians to participate in fights over scientific priority has come to seem increasingly pointless (though still hard to avoid).

Postpositivist accounts have, however, usually sought some refuge in other notions of objectivism. It is well known that Kuhn sought some sort of rationality in processes of theory change by appealing to such notions as simplicity. Alan Musgrave's account of the Chemical Revolution tried to fit it to the philosophy of Imre Lakatos, arguing that phlogiston theory and oxygen theory were, respectively, 'declining' and 'progressive' research programmes in the period 1770 to 1785.[24] Musgrave's approach remains objectivist in presuming that the identification of a programme of research as progressive or otherwise is something that can be done objectively, can be done by us now and was done by the protagonists in the late 1780s. As Musgrave might jokingly say, the protagonists eventually all read Lakatos, except Priestley, of course. Rejecting the timeless objectivity of the inductivists' crucial experiments, Musgrave substitutes a different form of objectivism based in the rationality of research programmes. The interpretation of the experiments on water is, on this view, either smoothly progressive, as with oxygen theory, or backward looking and regressive, as when Cavendish reverted to phlogiston as imponderable in the 1784 phlogiston theory deployed to account for the experiments in which water was produced.[25] Finding various 'external' explanations of theory change to be inadequate, Musgrave triumphantly offers us the rational choice of a progressive programme of research. The problem, of course, is that logic does not constrain judgements of what is progressive and what is not. Interpretative flexibility remains and the reasons why actors choose to pursue divergent interpretations still require explanation.

Postpositivist approaches have themselves come under criticism on a number of counts from sociological and so-called 'postmodernist' perspectives. Constructivist and postmodernist perspectives treat scientific change as a thoroughly constructed process that cannot be explained by appeal to reality or rationality as usually conceived. Notions of reality and rationality always require thorough contextual interpretation. The interpretative flexibility in both experimental set-ups and in any set of rules of method for conducting and interpreting them is never exhausted. We then look to other processes to curtail that interpretative flexibility. In the sociology of scientific knowledge, professional vested interests and/or wider social interests are invoked as explanations for the acceptance of certain interpretations rather than others, interpretations that eventually become routine, 'natural' and 'true'.[26] In actor-network theory, an attempt is made at a neutral, monistic account of the solidification of certain interpretations rather than others.[27]

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[24] Alan Musgrave, 'Why did oxygen supplant phlogiston? Research programmes in the Chemical Revolution', in Colin Howson (ed.), *Method and Appraisal in the Physical Sciences. The Critical Background to Modern Science, 1800–1905*, 1976, pp. 181–209.

[25] Ibid., pp. 204–205.

[26] See Barry Barnes, David Bloor and John Henry, *Scientific Knowledge. A Sociological Analysis*, 1996.

[27] The central text is Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society*, 1987. For important critiques see Steven Shapin, 'Following scientists around', *Social Studies of Science*, **18**, 1988, 533–50 and Simon Schaffer, 'The eighteenth Brumaire of Bruno Latour', *Studies in the History and Philosophy of Science*, **22**, 1991, 174–92.

As attention has been directed to the problem of interpretative closure, mechanisms involved in that process have come under greater scrutiny. The meeting of experimental, literary and social practices has become a focus of inquiry. Since the issue of language was so clearly announced as such by participants in the Chemical Revolution, it has naturally become an important focus for analysing such processes. From this perspective, the experiments on the composition of water become a key site where the linguistic and experimental practices of the new chemistry are woven together. The efforts of the French to effect this 'linguistic' revolution were still resisted to some extent by British investigators, even as they accepted what they took to be the major import of the Lavoisian scheme.[28]

With such constructivist approaches, the historiography of the Chemical Revolution links up conceptually with the attributional model of discovery as discussed in Chapter 2. Key discoveries in the Chemical Revolution become attached to Lavoisier through an active rhetorical process in which Lavoisier himself and his colleagues were directly involved as part and parcel of their scientific investigations. That rhetorical process then also 'spilled out' into secondary accounts of various sorts that were also of great importance to the attributional process.

In the case of Lavoisier, Bernadette Bensaude-Vincent's explorations of the founder myth and its construction provide a telling example of the way in which the detailed attributional processes of working science link up to the wider politics of reputation and scientific symbolism. Bensaude-Vincent shows that nineteenth-century French chemists paid inordinate attention to the history of their field and constructed a founder myth around the figure of Lavoisier.[29] Even before this, Lavoisier depicted himself as the progenitor of not just a revolution in chemistry but a *foundation* of the field. Lavoisier's studied neglect of history and the work of others in the *Traité élémentaire de chimie* was part of his claim to a radical innovation having no continuities with past work in the field. The founder myth also claimed for Lavoisier's system the status of template for the whole future development of chemistry. Even after Humphry Davy's discovery that acids need not contain oxygen and the rise of atomic theory seemed to challenge such a claim, French chemists, led by J.B. Dumas, preserved the Lavoisian founder myth.

In the nineteenth century, significant impetus was given to this myth making by international politics, especially the relations of France and Germany:

> [Lavoisier] seems to have become the emblem of French power, so that all the nationalist militancy stirred up between France and Prussia due to the politics of the day focused on his person. So it was that the legend of a chemist became one of the stakes in a complex game of political rivalry.[30]

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28 For a key text see: Steven Shapin, 'Pump and Circumstance: Boyle's literary technology', *Social Studies of Science*, **14**, 1984, 481–520 and on this intersection in late eighteenth-century chemistry see Wilda Anderson, *Between the Library and the Laboratory. The Language of Chemistry in Eighteenth-Century France*, 1984; Lissa Roberts, 'A Word and the World: The significance of naming the calorimeter', *Isis*, **82**, 1991, 198–222; Jan Golinski, 'The Chemical Revolution and the politics of language', *The Eighteenth Century*, **33**, 1992, 238–51.

29 Bensaude-Vincent, 'A founder myth in the history of sciences?', pp. 53–78.

30 Ibid., p. 65.

Bensaude-Vincent recalls Wurtz's famous statement in 1869 on the eve of the Franco-Prussian War that 'La chimie est une science française: elle fut constitutée par Lavoisier, d'immortelle mémoire.'[31] The responses from the German side represented shots in an ongoing propaganda war set against the reascendency of German chemistry in the 1850s, especially in the organic field.

There are a number of ways that the British arguments about Watt and Cavendish relate to these continental battles over the reputation of Lavoisier. First, although Lavoisier's system was fairly quickly adopted in Britain, the origins of the new chemistry were disputed. In particular, the importance of the British pneumatic tradition leading from Hales and Black through Priestley and Cavendish was asserted. British discussions of the discovery of the composition of water were overshadowed by what most took to be the engrossing tendencies of the French chemists with their state-sponsored New Chemistry and new nomenclature.[32] Cavendish and Blagden had to take steps in 1785 to assert Cavendish's priority over Lavoisier in the water experiments. British chemical texts of various sorts in the late eighteenth and early nineteenth centuries were routinely in the business of asserting Cavendish's claims against the ever-engrossing efforts of the French. As Lavoisier's accomplishment, and even the new nomenclature, were accepted, the British remained resistant to the idea that a Chemical Revolution had been effected in the sense that the French claimed.[33] The texts examined in Chapter 4 as an attributional survey have this as a constant backdrop. The British attempts to put forward Cavendish as their own candidate as the 'founding father' of chemistry would have created pressure against any claims that reduced Cavendish's reputation in any way. Hence came, perhaps, one source of pressure to neglect the claims of Watt to honours in the discovery of the composition of water, to leave them on one side, to contest them if raised. We will see also that British chemical texts of the later nineteenth century were still actively engaged in this struggle. By that time, although the defence of Cavendish's claims was still solid, there was a significant minority who even on the water question sided with Lavoisier as contests over methodology and pedagogy cut across entrenched nationalistic habits. As McEvoy noted, British historians of the late nineteenth and early twentieth centuries also participated in seeking to substitute an Anglo-Saxon 'positivist-whig' story for that promulgated by the French.

A second way in which the 'geopolitics' of Lavoisier's reputation is relevant to our story is through the involvement of a number of continental chemists in the Cavendish/Watt contest. Arago, though not a chemist, was clearly a major player.

[31] Ibid. The statement was made in C.A. Wurtz, *Dictionnaire de chimie pure et appliquée*, 1869, p. 1. For detailed discussions see Alan J. Rocke, 'Pride and Prejudice in chemistry: Chauvinism and the pursuit of science', *Bulletin of the History of Chemistry*, **13–14**, 1992–93, 29–40 and idem, *Nationalizing Science. Adolph Wurtz and the Battle for French Chemistry*, 2001.

[32] It is worth remembering here the discussions in Britain at this time of the issue of publication, the balance between public and private interests and how they were best served, a debate in which Watt participated informally. (See Jan Golinski, *Science as Public Culture: Chemistry and Enlightenment in Britain, 1760–1820*, 1992, pp. 41–44 and also Bensaude-Vincent, *Lavoisier*, p. 93).

[33] See David Knight, 'Revolutions in Science', in W.R. Shea (ed.), *Revolutions in Science. Their Meaning and Relevance*, 1988, pp. 50–51, 63.

J.B. Dumas, the French chemist who contributed so substantially to the Lavoisier founder myth, also participated and, interestingly, was claimed as a supporter by the Watt camp. A number of continental chemists and chemist–historians were courted by the Watt camp and expressed support for at least some recognition of Watt's claim. Although it takes us beyond the scope of the present work, and requires further investigation, it does seem likely that the decision to support Watt against Cavendish in the water question may have been the product of careful calculation by those chemists. In so far as Cavendish was put forward by many members of the British scientific élite as their candidate, against Lavoisier, for the title of 'founding father' of chemistry, then one can see why someone like Dumas might be happy for some of Cavendish's credit to be 'siphoned off', as it were, to an unthreatening figure like Watt. There may have been an element of this too with Arago, although we will see that he had other reasons for wanting to elevate Watt. It is possible also that German chemists and chemist–historians could have supported Watt in the belief that expanding the cast of characters with a claim to the discovery of the composition of water would, rather than elevate Lavoisier, act to bring him and his advocates closer to earth.

Although these aspects of the international politics of chemical mythology are not dealt with systematically in what follows, they do need to be borne in mind as a set of considerations possibly reinforcing, or cutting across, the domestic interest politics of chemical reputation that is investigated and emphasized. Now let us narrow our considerations from the broad historiography of the Chemical Revolution and chemical mythology to consider the place of the water experiments in chemical histories.

The Composition of Water in Histories of the Chemical Revolution

Charles Gillispie provides a classic example of the positivist treatment of the discovery of the composition of water in the Chemical Revolution. Gillispie argues that Lavoisier's work before 1783 was breaking loose from the chemistry of principles but carried with it, in the claim that oxygen is the acidifying principle, 'a diminishing residue of these old notions'.[34] Lavoisier had asked what is produced when hydrogen burns but he had found no product. At this point Gillispie introduced the English chemists to the scene. Priestley is depicted as mentioning the production of dew on the explosion of inflammable air with common air. However, Priestley was trying to determine the weight of heat. Cavendish repeated the experiment, made some measurements of how much of the air disappeared, and established that pure water was the product.

> In England James Watt … in France Gaspard Monge … made the same discovery, independently, almost simultaneously, and in the case of Monge with no less clarity. Once again, therefore, the interesting question is not who made water, but who understood what had happened? For though the composition of water was the last of the great discoveries of the English pneumatic school, Cavendish interpreted his results as the

[34] Charles C. Gillispie, *The Edge of Objectivity*, 1960, p. 227.

> condensation of water, not its synthesis. Indeed, the light weight of hydrogen encouraged certain of the English chemists, militant in their last ditch, to identify it for a time with phlogiston itself.[35]

The implication is that the English school discovered water only in the sense of stumbling across it materially. Stumbling is probably the right word since Watt is not granted the clarity that Monge (or Cavendish) is allowed. Whilst the English exploited the possibilities in this outcome for defending phlogiston, Lavoisier

> seized upon Cavendish's results as upon a piece of tactical fortune. Lavoisier … instantly saw water, seemingly the simplest of substances and classically the most intuitive of elements, rather as the oxide of that gas which is – to anticipate the rest of nomenclature – hydrogenerative. (Ibid., p. 228)

There followed Lavoisier's experiments before the Académie, the announcement of the results and, in due course, the experiment in analysis. Gillispie notes that Lavoisier made 'very fleeting mention of Cavendish', leaving the reader to suppose that this was the natural extension of Lavoisier's earlier work with hydrogen. Such a reader, he says, 'would be right, in principle if not in fact'.

> For this dramatic demonstration that water is an oxide, though not a 'crucial experiment' in the Baconian sense, was nevertheless decisive historically in the campaign to exorcise phlogiston in favor of a positive concept of combustion as that chemical reaction in which oxygen combines.[36]

Lavoisier's frontal attack on phlogiston 'made of chemistry a modern science', in the sense that it became a quantitative science: 'Henceforth, the chemist weighs amounts. He does not distil out principles.' Gillispie acknowledges that some historians do not see such a clear rupture, especially where Lavoisier's treatment of heat was concerned. Lavoisier's use of caloric (a subtle, elastic fluid and thus imponderable and all-permeating) means, according to Maurice Daumas,[37] that Lavoisier had not completely broken with the old system of 'principles'. Yet, Gillispie maintained, although Lavoisier uses the notion of caloric, he does not do so chemically: 'For caloric does not act as a chemical agent. It is excluded, absolutely excluded from the practice of chemistry, by the most elementary consideration of method.' Because heat (like light) is not confined within the boundaries of experiment, it cannot enter into the weighing of masses which has now become the essence of the science of chemistry. According to Gillispie, caloric 'entered permissively rather than constitutively into the structure of theory. Caloric disappeared from science in the nineteenth century. Nevertheless, the theory of oxidation, and by extension the conception of chemical reaction, remain what they were left by Lavoisier, and not by Priestley.' Gillispie's sleight of hand here is to treat caloric as an erroneous optional extra, only loosely tied to the core of Lavoisier's chemistry, which can be, and was, removed over time with *no change* in the central

[35] Ibid., p. 228.
[36] Ibid., p. 229.
[37] Maurice Daumas, *Lavoisier, théoricien et expérimentateur*, 1955.

theory. This is to translate the nineteenth-century meanings of the theory of oxidation back into the 1780s, rather than to inquire, as Daumas does, of the ways in which Lavoisier remained a man of the eighteenth century.[38]

Among those of more recent authors, Arthur Donovan's portrait of Lavoisier offers a concise account of the great Frenchman's development of the new theory of combustion. Donovan sees the 'Memoir on heat' produced with Laplace and read to the Académie in June 1783 as 'the final stone in the arch formed by Lavoisier's interrelated theories of acidification, calcination, combustion and respiration'. The composition of water then takes its place: 'Lavoisier then adorned this arch with his dramatic announcement that water, long thought to be an element, is in fact a compound formed by the combination of the two gases.'[39] What Donovan describes as Lavoisier's 'demonstration' of the compound nature of water conforms, he tells us, 'to the well-established pattern of British discovery and French explanation', a pattern transformed into a theme in modern historiography by Henry Guerlac.[40] All along, as Lavoisier had developed his other theories, Priestley had been characterized by Lavoisier as a most valuable experimentalist but lacking system and discipline. Donovan seems to share this view of Priestley, describing him as engaged in the 'apparently aimless ransacking of nature', and as 'prolific, chatty, and unsystematic'.[41] It is in this sense that Lavoisier is seen as giving theoretical coherence to the rather chaotic experimental discoveries of the British. It is acknowledged that when Lavoisier turned again to the burning together of hydrogen and oxygen, the experiment performed on 24 June 1783, much had already been achieved on the other side of the English Channel. Priestley had already reported finding water and Blagden had brought to Paris the news of Cavendish's quantitative finding that the weight of the water produced was the same as the weight of the gases burned. However, the complex, large-scale experiment that Lavoisier performed completed the construction of his 'comprehensive new theory'. Even as Lavoisier proceeded smoothly to this climax, 'false leads and confusion' were evident in Britain as Priestley created havoc with his claim to have converted water into a new kind of air by intense heat with quicklime. Watt and other of Priestley's friends are mentioned here as among those led astray, even as Priestley also announced his repetition of Cavendish's experiment of turning air into water. Priestley's retraction of his interpretation of the earlier experiment added to the confusion.[42] The impression we get is that the experiment performed in Paris on 24 June 1783 saw Lavoisier rise magisterially above those confusions (and above Cavendish's interpretative reticence) and show how the compound nature of water fit into the grand theoretical edifice that was now all but complete. Donovan does not explicitly award Lavoisier the *discovery* of the compound nature of water. Rather he writes of Lavoisier's *demonstration* of the fact, capped, of course, by the 1784 analysis and synthesis experiment. Donovan implicitly treats discovery as an empirical matter and reserves the term

[38] Gillispie, *Edge of Objectivity*, pp. 235–37.

[39] Arthur Donovan, *Antoine Lavoisier. Science, Administration, and Revolution*, 1993, pp. 153–54.

[40] Ibid., p. 154; Guerlac, 'Chemistry as a Branch of Physics', 193–276.

[41] Donovan, *Lavoisier*, pp. 139, 140.

[42] Ibid., p. 155.

'demonstration' for discoveries interpreted 'correctly'. This is a common and understandable approach in histories written from a French vantage point. Such a perspective has little or no place for Watt as someone who at best was a speculator hanging off Priestley's fraying coat tails.

A number of writers have addressed more directly the work done by Watt and Cavendish on the composition of water in the 1780s, have sought to interpret it, and to assess its significance for the resolution of the water controversy. Most of these writers have seen their historical task to be the resolution of the water question. In pursuing that objective they have inevitably read the work of Watt and Cavendish against the knowledge of what the composition of water came to mean in the years following Watt's and Cavendish's work. Broadly speaking, they have read it by reference to the impending Chemical Revolution.

One of the most comprehensive and accomplished treatments of the discovery of the compound nature of water is that provided by J.R. Partington in his multi-volume *A History of Chemistry* and also in an earlier monograph on *The Composition of Water*.[43] In his treatment of Cavendish, Partington takes us way beyond the simplistic accounts of most positivist-whig history. Partington finds Cavendish as a chemist 'a remarkably accurate experimenter' but 'lacking in ability to draw far-reaching generalisations from his results, which Lavoisier and Dalton possessed in a pre-eminent degree'.[44] Cavendish, Partington tells us, was the first to discover (in 1781) the experimental fact that 'hydrogen and oxygen gases (called by any names we wish), mixed in proper proportions (practically 2 vols to 1), can be completely converted into *their own weight* of water'.[45] Partington notes that we would expect Cavendish to conclude from this that the water had been produced from the gases and was a compound of them. But, he says, we need to appreciate that Cavendish remained under the influence of phlogiston theory. Partington explains Cavendish's meaning thus:

> He seems to have represented his results as follows. Let ϕ stand for phlogiston. Inflammable air is (water + ϕ), dephlogisticated air is (water – ϕ). When the two gases combine the reaction is:
>
> $$(\text{water} + \phi) + (\text{water} - \phi) = 2\ \text{water}$$
>
> The water pre-exists in both gases, and is deposited as a result of a redistribution of phlogiston. Cavendish's explanation is quite consistent with the view that water is an element.[46]

Partington recognized that this explanation on Cavendish's part is akin to the explanations given by others, though with the differences being crucial in some cases. This *sort* of deconstruction of Cavendish as discoverer of the compound nature of water had been engaged in by Berzelius, and by B. Paul in the nineteenth

43 Partington, *History of Chemistry*, vols 3 and 4, 1962, 1964; idem, *The Composition of Water*, 1928.

44 Partington, *History of Chemistry*, vol. 3, p. 312.

45 Ibid., p. 332.

46 Ibid., p. 334.

century.[47] George Wilson's deconstruction differed crucially in attributing to Cavendish the belief that hydrogen was phlogiston, in which case one quantity of water *was produced*, and the other set free, from the gases. Since on this view at least some of the water was produced, Wilson felt that this was a basis for attributing the discovery of the compound nature of water to Cavendish. Partington, however, disagreed since, he contended, Cavendish had dropped the idea that inflammable air was phlogiston, an idea he had entertained back in 1766. For Cavendish in 1784, phlogiston was, once again, imponderable. This being so, the above characterization of the reactions that he envisaged must hold, and we must acknowledge that in 1784 Cavendish still considered water to be an element. He was aware, as Partington tells us, that Lavoisier had an alternative explanation not involving phlogiston at all in which the gases united to form water as a compound. But Cavendish rejected this while still recognizing that experimentally the two explanations were difficult to distinguish.

Partington's treatment of Watt is also extensive and detailed. The overall impression, however, is of Watt's unfortunate reliance upon Priestley's confused, and confusing, experimental work, his lack of an experimental base of his own, and his long-continuing belief in the presence of water in all sorts of air.[48]

Partington provides us with a full account of the water controversy, but he is happy to take his conclusion from the work of Hermann Kopp, the German chemist and historian of chemistry:

> Kopp concluded that: 'Cavendish was the first to establish the fact from which the knowledge of the composition of water proceeded' but did not state the components of water; Watt 'first concluded from these facts that water is a compound, but without reaching a true knowledge of the components'; Lavoisier, from the same facts 'and with the recognition of the compound nature of water, first gave the correct determination and the exact statement of the components'. I think that this is the correct view.[49]

Partington thus divides the credit, or perhaps invites the reader to decide which is the most important contribution (the most important dimension of discovery) and to allocate credit accordingly. Partington expressed no preference himself. However, in his earlier discussion of these matters he was less generous to Watt's confusions and saw Lavoisier's first publication on the question in 1783 as 'simply a deduction' from Cavendish's experiments, seeming to elevate the importance of the latter.[50] He also placed the water controversy in a longer narrative, including earlier ideas about water and subsequent attempts to determine its composition ever more

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47 Ibid., pp. 334–35. J.J. Berzelius, *Traité de Chimie*, 1845, vol. 1, p. 354; B.H. Paul, 'Gas', in Henry Watts, *A Dictionary of Chemistry*, vol. 2, 1864, 773–82.

48 Partington, *History of Chemistry*, vol. 3, pp. 345–62. Watt's continuing 'erroneous' beliefs about water and airs are located in a letter to Black in 1788, and in Watt's contribution to the pamphlet (with Thomas Beddoes), *Considerations on the Medicinal use and on the Production of Factitious Airs*, 1794–1795, p. 114.

49 Partington, *History of Chemistry*, vol. 3, p. 359. Kopp's major treatment of the question is 'Die Entdeckung der Zusammensetzung des Wassers', in H. Kopp, *Beiträge zur Geschichte der Chemie*, Drittes Stück, 1875, pp. 237–310.

50 Partington, *Composition of Water*, 1928, pp. 31 (Watt's confusion), 36 (on Lavoisier).

accurately. The overall story he described as ‘an account of the experimental investigations leading to our present very exact knowledge of the composition of water’.[51] Partington, I think, admired the great experimentalist Cavendish and wished that he had been a little more prone to adventurous interpretation of results.

Partington takes us a considerable distance into a contextualized account of Cavendish’s, and Watt’s, understanding of the nature of water. However, another notable feature of Partington’s account is that his treatment of the water controversy is profoundly ahistorical. He acknowledges no phases in it, only the search for the facts about it pursued by many people over a period close to 200 years. Thus, in relation to particular points of interpretation, he gathered opinions and arguments from the 1780s, the 1840s and beyond. He himself debated with De Luc, with Wilson, Berzelius, Muirhead, Brougham, Arago, and so on. It is in this sense that I say that his account is ahistorical. Its ready deployment of chemical truths against which to judge participants’ work is another feature that places Partington’s account within the positivist-whig tradition identified by McEvoy. This despite the fact that Partington provides in many respects one of the most detailed and nuanced contextualizations available of the work of Watt and Cavendish on water. Although he is not one-eyed about Cavendish, Partington is very much in the interpretative tradition that descended from the writings of members of the Cavendish camp such as Harcourt, Whewell and Wilson. This is so because his primary concern is to discern the *meanings* of the contributions of the historical actors. There have, however, also been writings in the empirical tradition descended from the Watt camp’s approach.

Sidney M. Edelstein provided a classical example of this in a paper published in 1948. Decisively titled, ‘Priestley settles the Water Controversy’, this paper claimed that a letter that Edelstein had recently acquired settled the major scientific controversy.[52] Edelstein assumed throughout his account that the ideas of Watt and Cavendish about water were essentially the same. The question was treated as being about who knew what when. Specifically, Edelstein contended that the whole business could be reduced to the question of whether Cavendish’s communications to Priestley about his experiments also included *conclusions*. Both George Wilson and members of the Watt camp used arguments concerning this matter. Wilson contended that conclusions must have been communicated, and that Priestley was a pivotal figure because he would have known this. Muirhead argued, equally vehemently, that no such communication of conclusions took place, because there were no conclusions to be communicated.

Edelstein’s Priestley letter, dated 14 December 1783, was addressed to Sir Joseph Banks, President of the Royal Society. It was in response to a communication from Banks regarding a paper by Lavoisier, evidently on water. Priestley wrote:

> Mr. Watt is the person who is properly concerned in this business. For the idea of water consisting of pure air and phlogiston was his, I believe, before I knew him; and you will find it in the letter which he addressed to me, which was delivered along with the last paper which I sent to The Royal Society, but which he afterwards withdrew.

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[51] Ibid., p. vii.

[52] Sidney M. Edelstein, ‘Priestley settles the Water Controversy’, *Chymia*, **1**, 1948, 123–37.

> This letter he now wishes to put into the hands of Mr. De Luc, who will make some use of it. You will oblige us both, therefore, if you will give orders to have this letter delivered to Mr. De Luc, who will wait upon you for the purpose.[53]

Edelstein drew his conclusion from what this letter failed to say. Since water was mentioned as Watt's business, but no mention was made of Cavendish, Edelstein concluded that: 'No theory had been presented by Cavendish to Priestley, and apparently not to Sir Joseph Banks, on the composition of water before December 14, 1783.'[54] This conclusion becomes part of a chain of circumstantially based reasoning that, by placing Watt's theory as no later than April 1783, gave him priority. The claim that both Muirhead and Wilson had effectively nominated Priestley as the person best placed to arbitrate the question gives additional force to Edelstein's case.

I am not concerned to argue with Edelstein over this, although it should be noted that other interpretations are possible, notably that Priestley, far from just conveying his knowledge to Banks, was already making an interested *claim* for his friend Watt in the face of competition from other quarters. That might be why he does not mention Cavendish. Another point to note is that this exchange between Banks and Priestley occurred during the Royal Society dissensions of 1783–84, which, as we have seen, involved culturally based oppositions of which Cavendish and Watt were in many ways symbolic. So what Priestley said might well have been a claim prompted by these underlying tensions. However, the main point to be made at this stage about Edelstein's argument is that it is classically empirical in character. In complete contrast to Partington, the *meaning* of Cavendish's and Watt's conclusions scarcely enters the question at all for Edelstein. Everything is made to depend on who said what and when. It is simply assumed that if Watt's temporal priority can be established then his theory passes muster as the discovery.

Another historian who entered the fray in a broadly similar manner was Robert E. Schofield, well known for his work on the Lunar Society of Birmingham, which gave an unaccustomed prominence to the activities of provincial scientists and industrialists in the Industrial Revolution. In his work on the history of chemistry, Schofield can be seen in one sense as part of the postpositivist thrust. He resuscitated the reputation of Priestley as an important contributor to the Chemical Revolution rather than seeing him as just the reactionary face of the recalcitrant phlogistonists. Given this, it is understandable, perhaps, that Schofield also engaged in reviving Watt's claims to the discovery of the composition of water. The way that he did so, however, smacks more of the positivist concern with priority than the postpositivist project.

In a paper published in 1964 Schofield provided more epistolary evidence in the Watt cause.[55] He noted that Edelstein's publication had not induced any change in Partington's account and that general histories of science by A. Rupert Hall and others had been published in the interim which discussed the discovery of the compound nature of water without mentioning Watt at all. Edelstein's optimism

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53 Ibid., p. 123.

54 Ibid., p. 137.

55 Robert E. Schofield, 'Still more on the Water Controversy', *Chymia*, **9**, 1964, 71–76.

about resolving the controversy had been misplaced, Schofield said: ‘opinion still favors the claims of Cavendish and it has become obvious that the introduction of new evidence is unlikely to settle a controversy that primarily feeds on conflicting interpretation’.[56] Nevertheless, in the hopes of restoring Watt ‘to the status of an independent proposer’ of the theory, Schofield presented two further letters, this time from Priestley to J.A. De Luc.

The first letter, dated 13 December 1783, was undoubtedly the letter to De Luc that enclosed the letter to Banks dated 14 December 1783 that was published by Edelstein. Priestley’s letter to De Luc again stated that the idea about water now advanced by Lavoisier had occurred to Watt ‘a long time ago’. The second letter, dated 27 December 1783, also contained Priestley’s testimony on priority: ‘In my opinion Mr. Watt first entertained the idea you mention, and the experiment of Mr. Cavendish proved the justness of it, tho Mr. Cavendish had not that idea himself.’[57] The second letter also makes it clear, according to Schofield, that in arriving at his theory Watt relied on Cavendish’s experiments, not on Priestley’s. This is important because it is agreed that Priestley’s so-called repetition of Cavendish’s experiment on exploding inflammable and dephlogisticated airs involved an important difference. Priestley obtained his ‘inflammable air’ from charcoal, not from zinc. This we now know (as did participants in the second phase of the controversy) meant that Priestley’s experiment could not have produced the ‘right’ result quantitatively. Thus Watt relied upon Cavendish’s experiments, and, using them, arrived at the ‘right’ theory before Cavendish.

Schofield presented these letters as cutting past ‘the irrelevancies which have persistently crept into the argument’. These irrelevancies he identified as: ‘the personality of De Luc, Watt’s failure to accept the theory of oxidation (Cavendish did not accept it either), the tragi-comedy of misdatings and interpolations’. Schofield wanted to leave aside ‘useless and invidious remarks’ about the character of Cavendish, Blagden and Watt, and the ‘heated tones and recriminations of nineteenth century controversialists’.[58] In short, Schofield would have us put aside the substance of the whole second phase of the controversy as lost in irrelevancies. His conclusion is worth quoting at length:

> Cavendish caught the possible significance of some casual experiments made by Priestley and Warltire and instituted a series of experiments which might naturally lead him to a conclusion about the composition of water. Before he had arrived at that conclusion, however, or at least before he was sure enough to mention it, he reported on his experiments to Priestley. Priestley, attempting to adapt these experiments to a different purpose, reported them and reported the results to Watt, who then gave the experiment an interpretation, at least as unambiguous as that later published by Cavendish, stating the composite nature of water. This interpretation was formally written before the date of any written statement by Cavendish, but not, apparently, before a Cavendish interpretation

[56] Ibid., p. 71.

[57] Ibid., p. 74. ‘The idea’ here refers to Watt’s early notion that water could be converted into air by long-continued heating. The Cavendish camp treated the incorrectness of this idea as a disqualification. Schofield comments: ‘That the idea is incorrect is less important than the predisposition it would provide for Watt’s interpretation of Cavendish’s experiment’ (p. 75).

[58] Ibid., p. 75.

> was known to Charles Blagden. Watt's paper was withdrawn from publication when it became incongruous to the Priestley paper it had been written to accompany; Watt did not attempt to publish his claim until after Lavoisier's announcement of 'his' experiments and conclusions, evidently based on Blagden's report of Cavendish's work. When Watt learned of Lavoisier's papers, he attempted to establish his priority, but was then forestalled by the reading of the paper by Cavendish.[59]

This is an entirely plausible conclusion as to what occurred in 1783–84. If I were in the business of seeking to resolve the controversy, I would probably adopt something like this. But I do not seek resolution. What is notable about Schofield's account is that, like Edelstein's and like the account favoured by the Watt camp in the nineteenth century, it is a circumstantial one. Although Schofield takes more notice than Edelstein of matters of meaning, he still sidesteps them, as in the matter of Watt's early, but 'erroneous' theory and on the charcoal issue. His account is empirical in its primary reliance on testimony concerning timing and in its dismissal of rhetorical processes as irrelevant, useless and invidious.

It must be remembered, however, that although Schofield and Edelstein do battle with Partington and others on the water question, all share a similar perspective on the Chemical Revolution. The fight is worth fighting, precisely because of the perceived centrality of the discovery of the composition of water to the story of that Revolution.

It is perhaps stating the obvious to say that neither Watt nor Cavendish knew what was around the corner as they prosecuted their work on water. We need to try to recover what it was that they thought they were doing at that moment and then to see how they responded to later events, especially the articulation of the New Chemistry by Lavoisier. The protagonists themselves, and other chemists, had to decide in the later 1780s and 1790s how the work of Watt and Cavendish fit into the evolving story. The original impetus to, and meaning of, their work was transformed through this process of reinterpretation. The positivist-whig historiography was at work even then, assimilating Cavendish to the progressive story of chemistry and sidelining Watt. Fortunately, scholars examining the work of Watt's chemical mentor, Joseph Black,[60] and Watt's own work within the tradition that Black led, have given us many of the materials necessary to reconstruct the deeper chemical context of Watt's claims on the composition of water. Others, notably Jungnickel and McCormmach, have provided similar insights into Cavendish. It is to these that we now turn.

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59 Ibid., pp. 75–76.

60 Joseph Black (1728–1799) was Professor at Glasgow from 1756 and at Edinburgh from 1766. He and Watt worked together in Black's laboratory at the University of Glasgow, and maintained a correspondence for the rest of Black's life. See Eric Robinson and Douglas McKie (eds), *Partners in Science. Letters of James Watt and Joseph Black*, 1970.

James Watt, Joseph Black and the Scottish Tradition

Watt was working in a line of inquiry that did not relate directly to the problem of the chemical composition of water as we understand that problem, or, indeed, as the generations subsequent to Watt came to understand it. The issue of the nature of water was encompassed in Watt's intellectual universe, as in Joseph Black's, by the problem of heat. The composition of water was conceived as the reverse of what we would call decomposition, but what Black and Watt called 'inflammation'. This, as taught for many years in Black's lectures and set out, for example, in the 1797 edition of the *Encyclopaedia Britannica*, was one of a number of effects of heat.

The laboratory of Black and Watt, at Glasgow, was concerned in various ways with the heating of water, and especially with what we would call changes of state. The composition and decomposition of water was a part of this problematic for Black and Watt since it involved (in the changing of water into air and air into water) the same sorts of processes. Although history has separated Watt's work on the composition of water, on the one hand, and his experiments on steam in relation to his engine improvements on the other, there was an essential continuity between them. I suggest that Watt himself, and his friends and associates, made no such distinction.[61] Students at the University of Edinburgh attending Black's lectures and those of his successor, Thomas Charles Hope, also had some exposure to these continuities in the way Hope lectured about the 'experiments of Dr. Watt'. Within that Scottish tradition, at least, Watt was a chemist, not just because of his work with chlorine bleaching and alkali manufacture[62] but because of his work with Black, and on his own account, on heat.

Joseph Black's definition of chemistry in his early Edinburgh lectures was 'the Study of the effects of Heat & Mixture on Bodies and Mixtures of Bodies, with a view to the improvement of our knowledge'.[63] The topic of heat always began, and

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61 Much attention has been lavished, of course, on the relationship between Black's discovery of latent heat and Watt's improvements of the steam engine. Robison's writings on this question, though unreliable, have been enormously influential historiographically. (See Donald Cardwell, *From Watt to Clausius: the rise of thermodynamics in the early industrial age*, 1971, pp. 41–42.) Watt himself denied this connection in the sense that he repudiated the suggestion that he had been a pupil of Black. Watt did not deny, however, that the issue of latent heat and his steam-engine work were substantively related. In denying the specific indebtedness to Black, Watt, and his son after him, sought to make the great engineer's work completely *sui generis*. (See Chapter 5 below.) Hence my suggestion that in understanding Watt's intellectual world, his views on heat, his work on steam, and his ideas about the composition of water are best seen as all of a piece.

62 Later accounts of Watt tend to neatly divide his mechanical and his chemical exploits, and exemplify the latter by reference to chlorine bleaching and alkali manufacture. See, for example, A.E. Musson and Eric Robinson, *Science and Technology in the Industrial Revolution*, 1969, chs 8 and 10. Richard L. Hills, *James Watt. Volume 1: His time in Scotland, 1736–1774*, 2002 seems to maintain this distinction and makes little connection between Watt's work on heat with Black and his chemical interests. Watt is treated primarily as a 'chemical engineer' in his work on bleaching, alkali and for the Delftfield Pottery (pp. 143–79 and 303–11).

63 Lecture, 17 November 1766, in Blagden MS 'Notes of Dr Black's Lectures', 1766–67, Wellcome Institute for History of Medicine, as quoted in Henry Guerlac, 'Joseph Black's Work on Heat', in A.D.C. Simpson (ed.), *Joseph Black 1728–1799. A Commemorative Symposium*, 1982, p. 13.

took up a substantial portion of, Black's lecture courses in chemistry. Black's first chemistry lectures were delivered in Glasgow in 1757–58, and his association with James Watt dates from this time also. By the mid-1760s John Robison had joined the circle and Black was delivering lectures that the best of his students perceived to be highly original, especially in regard to Black's discovery of what came to be known as 'latent heat'.

Black's work on heat had a number of roots. It was partly inspired by William Cullen's experiments on evaporative cooling and partly by meditations on processes of freezing. Simple facts, such as the longevity of snowfalls upon the ground even when the temperature rose above freezing, convinced Black that processes of freezing and vaporization involved large quantities of heat well beyond those that ought to be required according to prevailing ideas. His first major experiments on raising the temperature of water and ice were conducted in 1761, and a year later he performed experiments on vaporization. From these experiments, values were obtained for the quantity of heat required for (what would later be called latent heat of) fusion and vaporization.

Watt became involved in this work first through his famous efforts with the model of a Newcomen Engine undertaken as part of his duties as instrument maker to the University of Glasgow. In the winter, spring and summer of 1764 Watt performed a series of experiments on steam. He was able to show that steam could heat about six times its own weight of water to boiling point. In the working of the Newcomen Engine this manifested itself in the large quantities of cold water necessary to condense the steam in the cylinder. When this result was mentioned to Black, and Watt learned of the idea of latent heat, he realized that this explained the phenomenon of the steam engine. Watt's crucial move to the separate condenser, which he made in 1765, was inspired by these experiments. Black and Robison were to claim that Black's work was Watt's inspiration, but the engineer himself denied that this was the case, maintaining that he arrived at the idea independently of learning about Black's notion of latent heat.[64] Watt, however, did not deny his general debt to Black.

It is true, and most important to our current concerns, that from 1764 Watt's work on heat was undertaken as part of Black's tradition of work.[65] Black benefited from Watt's experimental acumen and skill. Watt surely benefited as Black and his students turned at least some of their experiments in directions suggested by, and relevant to the task of, improving the steam engine. Black drew an explicit connection between accurate determination of the latent heat of steam and the measurement of how well fuel was being used. Watt drew inspiration from work on the lowering of the latent heat of vaporization in situations of reduced pressure in pursuing plans to save fuel by operating at such reduced pressures, but his experiments showed the ultimate futility of any such scheme. The experimental work on determination of specific heats undertaken at Glasgow before Black's departure for Edinburgh in 1766 was closely connected with Watt's search for metals and alloys that would enable a cylinder to be made that absorbed less heat.

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[64] See Arthur Donovan, *Philosophical Chemistry in the Scottish Enlightenment. The Doctrines and Discoveries of William Cullen and Joseph Black* , 1975, esp. pp. 250–64.

[65] The following relies heavily on Guerlac, 'Joseph Black's work on heat', pp. 13–22.

All this is by way of enabling us to comprehend properly the roots and the character of Watt's chemical work. For him, as for his collaborator Black, the phenomena of heat were central to *chemical* processes. The focus upon steam and upon processes of its vaporization and condensation significantly shaped the ideas that Watt brought to the chemistry of airs. In so far as this connection can be made, Watt's 'speculation' on the composition of water in 1783 can itself be placed in a tradition of work. This opens the possibility that it was not a 'random' speculation or an inspired hunch without any sustained bases, as the anti-Watt, pro-Cavendish camp sought to portray it. There was, then, a coherent basis on which Watt's claim *could have been defended*. That it was not seriously defended during the first phase of the controversy was probably due to the fact that most British chemists, including, crucially, Black himself (and Thomas Charles Hope, his successor at Edinburgh), acceded quite quickly to the New Chemistry of Lavoisier, as can be seen in the shifts taking place in the lectures at Edinburgh.[66] Watt's achievements were lauded in those lectures. His experiments on steam and vaporization survived intact. However, his contribution to pneumatic chemistry paled, and was then excised unnoticed, as the all-powerful Lavoisian scheme came to dominate.

One of the few authors to set Watt's chemical work into a carefully observed context was David R. Dyck.[67] Dyck set himself the task of examining the relationships between conceptions of heat and chemistry during the eighteenth century. He makes a number of very important points about Watt's work on steam and water.

First Dyck reminds us that Watt's understanding of, and explanation of, the composition of water involved an integral role in the chemical process for latent heat. In this Watt went beyond Black's own work, since Black did not really attempt an integration of his physical work on heat and other aspects of his chemistry. The two sat side by side. They were conceptually linked for Black, but he did not actively mobilize the heat concepts to solve chemical problems as conventionally recognized. However, Watt, and other of Black's students and followers (such as Crawford, Cleghorn and Irvine, who were active in the development of ideas about heat) brought them together. Second, Watt's explanation of the composition of water had also grown out of ideas about the relationships of water, air and fire that existed in older traditions and works. He long believed that if water could be heated enough, it would change not just into steam but ultimately into air.

Watt's work on the composition of water thus was part of a 'Scottish School' of chemical work deriving from, but also in some ways departing from, the work of Black. That strand, and its distinctiveness, have been virtually lost sight of because of the readiness and near-totality with which Lavoisier's interpretation was adopted in Britain and, most importantly, because of the way chemists and commentators

[66] On the Scottish response to Lavoisier see Arthur Donovan, 'The new nomenclature among the Scots: Assessing novel chemical claims in a culture under strain', in Bernadette Bensaude-Vincent and Ferdinando Abbri (eds), *Lavoisier in European Context. Negotiating a New Language for Chemistry*, 1995, pp. 113–21. On Black see also C.E. Perrin, 'A reluctant catalyst: Joseph Black and the Edinburgh reception of Lavoisier's Chemistry', *Ambix*, **29**, 1982, 141–76.

[67] David Ralph Dyck, 'The nature of heat and its relationship to chemistry in the eighteenth century', unpublished PhD thesis, University of Wisconsin, 1967.

retrospectively assimilated Black's work, seen as a *physical* account of heat, to the new system. Once that had been done, Watt's integration of Black's heat theory into chemical processes began to look like a backward step, if it was recognized at all. Those taking Watt's chemical statements seriously in the second phase of the water controversy inevitably found them archaic and outlandish in the way that they incorporated latent heat into the chemical process proper. Whewell put it clearly when he said:

> Watt's views are utterly damaged by involving a composition of ponderable and imponderable elements. This of itself was enough to show that he did not consider elementary composition with the rigour and distinctness which the discovery of the synthesis and analysis of bodies at that time required.[68]

In this way it was much easier to ignore the discredited tradition within which Watt worked and to sideline him as merely engaged in speculative inference on the basis of the experiments of others when he ventured his views about the nature of water.

The view that if only steam could be heated enough then it would be converted into air, just as water if heated enough was converted into steam, was an idea with a history, and in its general form Watt could have acquired it from Eller,[69] who suggested that water is converted into air by combination with elementary fire. The early decades of Watt's interest in chemistry and heat (the 1760s and 1770s) saw a proliferation of ideas about entities actively involved in chemical reactions including not only phlogiston but also heat (fire) and light.[70] Watt characterized his long-held views on the relations of water and air in a letter to Matthew Boulton in 1782:

> You may remember that I have often said that if water could be heated red-hot or something more, it would probably be converted into some kind of air, because steam would in that case have lost all its latent heat, and that it would have been turned solely into sensible heat, and probably a total change of the nature of the fluid would ensue.[71]

Watt believed when he wrote this letter that Priestley had performed an experiment that proved that this change did indeed happen.

Watt expressed the same idea in a letter to Black at the same time.[72] The context in which he did so is important. Watt had been advised by De Luc of the latter's intention to write something on heat. De Luc was seeking information from Black, and Watt believed that De Luc would try to do justice to Black's claims to the discovery of latent heat. Watt explained to Black that he would provide De Luc with an account of his own work. This would be difficult because Watt sometimes found it hard to distinguish between 'the suggestions of my own mind' and 'what I

68 Whewell to Harcourt, 11 February 1840, in *The Harcourt Papers*, vol. xiv, pp. 105–106.

69 Johann Theodor Eller, *Physikalisch-Chymisch-Medicinische Abhandlungen aus den Gedenkschriften der König*, 1764.

70 See Hélène Metzger, *Newton, Stahl, Boerhaave et la Doctrine Chimique*, 1930.

71 Quoted in J.P. Muirhead, *The Origin and Progress of the Mechanical Inventions of James Watt*, 3 vols, 1854, vol. 2, p. 167.

72 James Watt to Joseph Black, 13 December 1782, transcribed in Eric Robinson and Douglas McKie (eds), *Partners in Science. Letters of James Watt and Joseph Black*, 1970, pp. 117–19.

have learnt from you'.[73] Watt then explained what experimental results he considered to be his own, the theoretical ideas that had grown out of them, and how Priestley had provided experimental support:

> What I mean to tell him that I think my own, is the trying the experiment on the latent heat *in vacuo* and the finding it to be greater than under the pressure of the Atmosphere – The expts to ascertain the different degrees of heat at which water boils under different pressures – the expansion which steam in its perfect state receives from heat and the experiments on the bulk of water when converted into steam together with a theory which I have devised which accounts for the boiling heats of the water not following a Geometrical progression, and showing that as steam parts with its latent heat as it acquires sensible heat or is more compressed, that when it arrives at a certain point it will have no latent heat and may under proper compression be an elastic fluid nearly as specifically heavy as water and at which point I conceive it will again change its state and become something else than steam or water. My Opinion has been that it would then become air, which many things had led me to conclude and which is confirmed by an experiment which Dr Priestley made the other day in his usual way of Groping about … [74]

Watt also recounted this phase in his intellectual history in his 'Thoughts on the Constituent Parts of Water' as published in the *Philosophical Transactions*, which, of course, took the form of a letter to De Luc dated 26 November 1783.[75] He explains how he reached the view that steam might be changed directly into air if only enough heat were supplied to it:

> This opinion arose from a discovery that the latent heat contained in steam diminished in proportion as the sensible heat of the water from which it was produced increased; or, in other words, that the latent heat of steam was less when it was produced under a greater pressure, or in a more dense state, and greater when it was produced under a less pressure, or in a less dense state; which led me to conclude, that when a very great degree of heat was necessary for the production of steam, the latent heat would be wholly changed into sensible heat; and that, in such cases, the steam itself might suffer some remarkable change.[76]

The point of Watt's paper in the *Philosophical Transactions* was, of course, that he now abandoned this opinion for the theory that became his claim to the discovery

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73 Ibid.

74 Ibid., at pp. 117–18. The account continues by describing Priestley's experiment: 'as he had succeeded in turning the acid into air by heat only, he wanted to try what water would become in like circumstances. He under saturated some very caustic lime with an ounce of Water, and subjected it to a white heat in an earthen retort, he fixed a Balloon between the receiver and the retort, No water or *moisture* came over but a quantity of Air = in weight to water viz. 200oz measures, a very small part of wch was fixt air and the rest —of the nature of Atmospheric air but rather more Phlogisticated, he has repeated the experiment with the same results … '.

75 James Watt, 'Thoughts on the constituent parts of water and of dephlogisticated air; with an account of some experiments on that subject. In a letter from Mr. James Watt, Engineer, to Mr. De Luc, F.R.S.', *Philosophical Transactions*, **74**, 1784, 329–53.

76 Ibid., p. 335.

of the composition of water. What must be noticed, however, is that Watt's thinking and ideas about the relations of water and air go back some way; that, primed probably by ideas on water/air conversion by earlier authors, he had been entertaining such ideas for some time during his experiments on steam. It can also be seen that whilst his abandonment of his earlier views was a significant break, there were also deep continuities, especially in the centrality of heat to the chemical process that he envisaged.

As Dyck argues, Watt's distinctive chemical explanation in his work on the composition of water (as well as the distinctiveness of that by Crawford, Cleghorn and Irvine) was effectively submerged by the assimilation of Black's work to the Lavoisian system in the 1780s and 1790s. Watt had taken up and pursued some of the possibilities that Black's doctrines offered for explanation of chemical change in terms of latent heat, possibilities that Black himself had not pursued. Once those doctrines were subsumed into another system, Watt's work would come to seem peculiar and misdirected if examined in detail. Even by the late 1780s and 1790s this process was well under way. There were some 'lags', however, where assimilation into the Lavoisian system was resisted and where something of the Scottish tradition in its unassimilated form can be seen. Articles in the third edition (1797) of the *Encyclopaedia Britannica* written by John Robison are an important example. As is well known, Robison, for political reasons, resisted the lure of system that he and others saw as characteristic of French chemistry.[77] We will see in the next chapter how Robison's articles, fiercely defensive of the empirical approach that he attributed to Black, preserved as if in amber some of the meanings and understandings of the unassimilated Black tradition, not least of these the language of the transformation of water into air by the application of heat.

Watt himself did not participate in the public discussions concerning water or in the public debate between supporters and opponents of phlogiston during the later 1780s and the 1790s. This lack of an ongoing presence in the debates may explain why Watt's claim to discovery was largely overlooked, even by students taught by Black and Hope in Edinburgh during that period.[78] Watt had other preoccupations and these, together with his well-known diffidence, may explain his lack of continuing involvement.

There were, however, two contexts in which Watt did return publicly to the chemistry of airs. The first was in his cooperation with Thomas Beddoes and the latter's Pneumatic Institution. Watt's interest in the medical uses of airs had been prompted in part by the illness of his daughter Jessie, who died in June 1794. In a joint publication with Beddoes, issued first in 1794–95, there is a letter by Watt, dated 2 September 1794, in which he makes the statement that 'no species of artificial air is obtained except water is obviously present, or that there is reason to suspect it may be contained as an element, or part of one of the substances

[77] See Donovan, 'New nomenclature', pp. 119–20 and J.B. Morrell, 'Professors Robison and Playfair and the *Theophobia Gallica*: Natural philosophy, religion and politics in Edinburgh, 1789–1815', *Notes and Records of the Royal Society of London*, **26**, 1971, 43–63.

[78] See James Kendall, 'The first chemical society, the first chemical journal, and the Chemical Revolution', *Proceedings of the Royal Society of Edinburgh*, **63A**, 1952, 346–58, 385–400. The issue of the Edinburgh 'neglect' of Watt's claim is discussed in the attributional context in Chapter 4.

concerned … '.[79] This is redolent still of supposedly superseded ideas that water is contained in gases. In the 1796 edition Watt described a pneumatic apparatus of his own design for producing factitious airs in quantity. He also described the preparation of 'Inflammable or Hydrogene Airs'. In this category Watt included gases made from zinc and iron by the action of water, from charcoal heated to redness, and water (producing what he calls 'heavy inflammable air') and also gases produced by heating animal substances (what Watt calls 'animal inflammable air').[80] As Partington notes, Watt seems at this time, over a decade after such gases were carefully distinguished in the work of Cavendish and Lavoisier, to have still regarded them all as varieties of one kind of inflammable air. This also suggests that Watt long continued to think of generic airs distinguished by their combinations with elementary heat. Humphry Davy reported from conversations that he had with Watt and Keir in 1799 that they were both still phlogistians, this at a time when the recalcitrant Priestley, in American exile, is usually considered the last bastion.[81] The second, more remote, context in which Watt revisited his views on water occurred during his editing in 1814 of his friend Robison's *Encyclopaedia Britannica* articles on 'Steam' and 'The Steam-Engine' for republication. The significance of this production lies mainly elsewhere in that it is direct testimony from Watt about his route to his steam-engine improvements. However, his revision also contains an account of his experiments on steam and those of his associates. Here Watt finally laid to rest the notion that if all the latent heat of steam could be converted to sensible heat, then the steam would become air. His later experiments on steam showed that the grounds on which he saw the possibility of this interconvertibility of latent and sensible heat were unfounded. Watt's recognition of this constituted his final discarding of the central pillar upon which his views on water had been built. It appears likely that Watt saw himself as a chemical interloper who lacked the experimental wherewithal to continue involvement in the strenuous battles of the 1780s and 1790s.

It should be noted that the second phase of the water controversy in the 1830s to 1850s (and indeed the attributional processes operating between the two phases) involved at various levels the suppression, or selective highlighting, of the contextual account of Watt's ideas on water that we have painted here. The Watt camp, whose members argued their case primarily using an empirical model of discovery (that of synonymity and priority), sought therefore to minimize the difference between the conceptions of the composition of water held by Watt, Cavendish and Lavoisier. They sought to treat the ideas of Watt and Cavendish, in particular, as essentially the same. They were aided in this by the processes that encouraged assimilation of Black's work, and that of his followers, to the same intellectual universe as the

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[79] Thomas Beddoes, *Considerations on the Medicinal Use and on the Production of Factitious Airs*, 1794–95, Part II, pp. 19–22, at p. 20. On Watt's work with Beddoes see Dorothy A. Stansfield and Ronald G. Stansfield, 'Dr. Thomas Beddoes and James Watt: Preparatory work 1794–96 for the Bristol Pneumatic Institute', *Medical History*, **30**, 1986, 276–302.

[80] Beddoes, *Considerations on the Medicinal Use and on the Production of Factitious Airs*, 3rd edn, enlarged, 1796, pp. 212–15.

[81] Humphry Davy to Davies Gilbert, 22 February 1799, cited in A. Clow and N.L. Clow, *The Chemical Revolution*, 1992 [1952], p. 496.

progenitors and supporters of the New Chemistry. In so far as this assimilation was achieved, it became possible to argue the circumstantial case for priority in terms of dates. Cavendish supporters, on the other hand, had every incentive to reveal the *dissimilarities* between Watt's work and conceptual universe and those of Cavendish and Lavoisier. Hence the emphasis in Harcourt's, Whewell's and Peacock's writings, for example, upon Watt as unaligned with the directions in which the science of chemistry 'had' to progress. Their attempts to restore Watt to his intellectual context were not inspired primarily by a search for good historical practice but by the need to place him outside the main line of development of chemistry as a realm of investigation.

Cavendish's Route to Water

The historical accounts of Cavendish's work on water which came down to modern historians from the water controversy tended to assimilate Cavendish to the Lavoisian Revolution in chemistry. Recent scholarship, however, has begun to thoroughly recontextualize it. The recent biographers of Cavendish, Jungnickel and McCormmach have made important contributions in this regard.

We have already seen to some extent, in Partington's account of Cavendish, the contextualization of his experimental work on water. It is very doubtful that Cavendish considered that water was not an element during the course of his experimental work. His basic idea was that water 'condensed' during the explosion of the mixed gases rather than being formed. Only in later years was his experimental work extracted from this theoretical context and placed into another (Lavoisier's) in which water is, indeed, formed as a compound. Similarly Cavendish considered his 'Experiments on Air' as providing a key element in the defence of phlogiston theory against the antiphlogistians, and many did see it this way.

There is, however, another sense in which Cavendish's work requires contextualization. This has to do with the aims of his research. Once we place the 'Experiments on Air' in a longer train of inquiry we begin to see what it was that Cavendish considered himself to be doing. Cavendish's interest lay, as he announced at the beginning of the first paper, in the various ways in which common air was phlogisticated. Such phlogistication was the consumption of 'good' air. Activities such as breathing common air, burning things in it, and processes such as putrefaction of animal or vegetable material, were all ways of phlogisticating the air.

Joseph Priestley had done important work on the goodness of air. In a paper published in 1772[82] he reviewed the field of pneumatic chemistry, announced the discovery of a new kind of air (nitrous air) and showed how the latter could be used as a measure of the goodness of common air. Nitrous air phlogisticated common air when mixed with it and Priestley developed this into a test of goodness using a device known as a 'eudiometer' to measure the diminution of the common air. The design of new, improved eudiometers and their use to measure the goodness of the

[82] Joseph Priestley, 'Observations on different kinds of air', *Philosophical Transactions*, **62**, 1772, 147–264.

air in various places and conditions became a popular area of inquiry, and an area of strong medical interest, in the late 1770s and early 1780s.[83]

Before the appearance of his first paper on ‘Experiments on Airs’ in 1784, Cavendish had published in the *Philosophical Transactions* an account of a new eudiometer and its use in measuring the goodness of air. He reported numerous trials of samples of air taken in London and Kensington in a range of weathers and at different times of day. From these he concluded that the degree of phlogistication of the air did not vary between places or times. On the basis of these measurements he also arrived at a figure that we can interpret as a measure of the concentration of oxygen in the atmosphere. Much has been made of the accuracy of Cavendish’s figure of 20.83 per cent.[84]

Now we can see the ‘Experiments on Air’ in a new light. As Jungnickel and McCormmach put it:

> We might expect that just as he [Cavendish] and Black had replaced the ancient element air with distinct gases, he would show that the ancient element water was a combination of gases, but that is not what he thought he did. He did not bring into question the elemental notion of water, even as his experiments laid the factual basis for our modern understanding of water as a chemical combination of gases. His way of referring to water was ambiguous as to its elemental or compound nature … [85]

Cavendish’s eudiometry experiments were the most important of a range of inspirations for his inquiry into the modes of phlogistication of common air, of which the explosion of air with inflammable air by an electric spark in a closed glass vessel was one. Cavendish was in fact critical of medical eudiometry and his intervention is usually seen as important in the demise of that tradition. His ‘Experiments on Air’ involved a shift of eudiometric devices from the medical context to the investigation of combustion.[86] The production of dew had been observed by Warltire and Priestley, whose work Cavendish was repeating. However, Cavendish went further. His observation was that the dew condensed out of the inflammable air and approximately a one-fifth part of the common air. This was not the *production* of water but more like its release. When dephlogisticated air (which Cavendish regarded as water deprived of its phlogiston) was exploded with inflammable air (which he considered to be either pure phlogiston, or more likely, water united to phlogiston), the deprivation and surfeit of phlogiston effectively cancelled each other out to produce pure water. The elemental character of water was unchallenged in this process

We can see, then, that there was considerable retrospective reorientation involved in locating Cavendish in a single progressive line as part of the ‘logic’ of the Chemical Revolution. Some of this reorientation occurred thanks to Cavendish

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[83] On the medical and social reform context of eudiometry see Simon Schaffer, ‘Measuring virtue: Eudiometry, enlightenment and pneumatic medicine’, in Andrew Cunningham and Roger French (eds), *The medical enlightenment of the eighteenth century*, 1990, pp. 281–318.

[84] Christa Jungnickel and Russell McCormmach, *Cavendish*, 1999, pp. 357–59.

[85] Ibid., pp. 362–63.

[86] Schaffer, ‘Measuring virtue’, pp. 312–13.

himself during his ongoing participation in chemical debate; some was accomplished on his behalf by Blagden. The rest can be located in attributional processes during the later eighteenth and very early nineteenth centuries. By the time of the second phase of the controversy there was little attempt, even by the Watt camp, to restore Cavendish's work to its original context. The closest that participants came to this was to reassert (as did David Brewster, for example) that there was no essential difference in the extent to which both Watt and Cavendish were in the thrall of phlogiston theory.

Conclusion

We have been concerned in this chapter with the 'action' in the 1780s concerning understandings of the nature of water. That action became, of course, the central focus of the second phase of the water controversy. It has, in more recent times, been incorporated into historical writing on the first phase of the water controversy and, more broadly, on the Chemical Revolution.

The significance attributed to various pieces of the action depends upon historiographical assumptions (positivist-whig, postpositivist, 'postmodern') and, more specifically, upon the model of discovery adopted. The confident stipulations of the positivist-whig approach read backwards from the rights and wrongs of the New Chemistry in its mature formulations and find matches or mismatches in the case of the work of Lavoisier, Cavendish and Watt. Whilst the work of Lavoisier and Cavendish has been readily assimilable to the mainline story of the Chemical Revolution and the place of understandings of water in that story, Watt has been marginalized. Watt is dismissed as engaged in ungrounded speculation or as having ideas rooted in 'failed' traditions. Postpositivist and postmodern historiographies avoid such confident stipulations, although they tend to persist in telling the mainline story in a different way. Lavoisier, and Cavendish to a lesser extent, are still the focus but their relative standing as discoverers emerges not from the inherent validity of their ideas, but from sociological and rhetorical processes. There has been no serious attempt to reinsert Watt into the picture. Whilst this would be possible, it is not my aim here to do more than hint at the possibility. The fact remains that Watt was written out of the picture. In the next chapter I exhibit some of the processes whereby this came about in the years before the attempt to revive his claim.

Chapter 4

Attributional Survey: Phase One, 1784–1830

> When the theory of the composition of water was spoken of in the presence of my father, he calmly but uniformly sustained his claim to its discovery; and once, on my hinting that it was passed over by some writers, and not correctly given by others, he observed … *he should leave posterity to decide*.[1]

Introduction

This chapter reports on the way in which scientific publications of various types discussed, more often merely mentioned, the discovery of the compound nature of water (and who made it) during the period when the controversy proper was quiet. The attributional approach to discovery involves the idea that cumulative usage is just as important, if not more important, than explicit argument in establishing discovery claims. The chapter examines encyclopaedia articles as well as scientific texts, especially, but not solely, chemical ones, and also survey histories and popular representations of the main protagonists. The circumstances of the production of some of these attributions will also be sketched. The survey enables an assessment to be made of the 'gradient' or the distribution of credit for discovery that prevailed when the second phase of the water controversy began.

From the perspective being taken in this book, James Watt's attitude to the issue of credit for discovery of the theory of the composition of water, expressed in the epigram for this chapter, was paradoxical. The son recalls his father wanting to 'leave posterity to decide', and yet being indifferent to what writers were saying about the question. Yet what is posterity if not what writers *in the future* will say? That will surely be conditioned, if not determined, by what writers have said in the interim. Of course, Watt's attitude makes sense if we assume, as is most commonly done, that it is possible for posterity to decide on the 'facts' of the matter independently of what others have said. If the 'truth' is accessible independently, then Watt's attitude is justifiable. We have seen, however, in our discussion of the nature of discovery, that there is much to be said for an attributional model of that process. On that view, the discoverer is the person attributed with the discovery by his/her ongoing community. This, I suppose, is what posterity is. Attributions create the discoverer, and the discovery.

[1] 'Letter from James Watt Esq., to the Editor', in James Patrick Muirhead (ed.), *Correspondence of the late James Watt on his discovery of the theory of the composition of water*, 1846, pp. iii–iv.

I have drawn a distinction between a first phase and a second phase of the water controversy. In the first phase the claimants themselves were involved. The second phase, involving various parties arguing on behalf of the long-dead claimants, occurred some fifty to seventy years later. During the interim there were few public signs of the controversy, but this does not mean that nothing was happening. In various more or less public utterances, interpretations were being offered of the meaning of the controversy. This might be done by the author of a dictionary who stated, for example, that Cavendish discovered the composition of water. Such a statement, by what it said and by what it left out, concretized and simplified the historical process in a way that favoured one interpretation and rendered it as 'fact-like'. Numerous statements of that type fed into the perception of the issue within the scientific and wider cultures. Since such processes were occurring between what I have called the phases of the controversy, the controversy is, in a sense, a continuous process. The appearance of phases is created by a hiatus in the explicit declamatory contributions that we normally associate with controversy.

Attributional processes between the 1780s and the 1830s are important because, as I will argue here, they seemed to place Cavendish in a stronger position than Watt by the later period. In the second phase of explicit controversy the supporters of Watt started from a difficult, but perhaps not impossible, position. We will see that the pattern of attributions had created what we might call an interpretative gradient in the culture that made arguing the case for Watt an uphill battle.

Although the water controversy as such was relatively quiet during this interim period, other issues concerning the history of chemistry were, of course, much in agitation. Even as the victory of the antiphlogistians was substantially conceded in Britain and elsewhere, the significance of the New Chemistry and its origins were still debated. As David Knight,[2] among others, has shown, practitioners and writers of chemistry during the French wars and beyond betrayed little sense of any revolutionary change in the field, let alone of a recent *foundation* of it. They sought to distance their subject from revolutionary implications (whether associated with the French or with Joseph Priestley) and to demonstrate that it was not a French science. The latter demonstration involved competition in research, certainly, and the accomplishments of Humphry Davy and of John Dalton conspicuously contradicted key aspects of the Lavoisian system, namely, that oxygen was the sole acidic principle and that the chemistry of atoms was useless metaphysics. The demonstration also involved writing the history of the developments in the New Chemistry in a way that downplayed the system-building of the French and emphasized the virtues of inductive inference and avoidance of speculation. Alternative lineages for the development of the science made much play of the contributions of Joseph Black and Henry Cavendish, in particular. Their research could be shown to have many of the virtues claimed exclusively by the more rabid Lavoisians, including great precision and quantification, as well as some, like caution, that had somehow been overlooked by others. It must be remembered as we proceed that the texts that we examine below were almost invariably engaged in these sorts of ideological battles.

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[2] David Knight, 'Revolutions in science: Chemistry and the Romantic reaction to science', in W.R. Shea (ed.), *Revolutions in Science. Their Meaning and Relevance*, 1988, pp. 49–69.

In studying processes of attribution we rely necessarily upon the examination of relevant texts, in this case treatises on chemistry, dictionaries of arts and sciences and, importantly, encyclopaedias. These texts relate somewhat differently to different audiences. There is, for example, the audience of active chemists, what Harry Collins calls the 'core set'.[3] The members of the core set are significant because they are recognized 'experts' in the area. Their attributions will carry particular weight and may even settle a controversy if others bow to their verdict. However, the literature relevant to opinion formation among the core set, and expressing their views, was broader in the early nineteenth century than it would be now. Thus expert chemists would both write for and read major encyclopaedias of the period.[4] Encyclopaedias were not purely popular publications. Understandings beyond the core set, however, are also important when people struggle not just over scientific credit but also over iconic status in the culture. So representations in the more popular literature also need to be considered.

Whilst published texts are the most central materials to be surveyed from an attributional perspective, other documents can be informative. The informal communication system is known to be as important as the formal one in the development of scientific opinion. The content of chemical lectures is of obvious interest here. What were students of chemistry, such as the numerous medical students taught chemistry at the University of Edinburgh, told about the composition and decomposition of water during their lectures? Monuments provide a further possible 'text'. It appears, for example, that Watt Jr came close at one point to commemorating his father's 'discovery' in the inscription upon the monumental statue created to Watt's memory in Westminster Abbey.[5] In that way an attribution might be literally 'carved in stone'. Such processes are, however, rare, and their permanence perhaps more than balanced by their immobility, and therefore inaccessibility to large audiences. The disappearance of lectures into the ether, except in private students' notes, also limits their circulation. Hence the power of the *printed* word.

Chemical Treatises and Texts

Systematic treatises and texts of chemistry began to proliferate in the late eighteenth century, variously reporting and summarizing research in the field and increasingly offering systematic surveys of it for students at all levels.

First there were texts through which the merits or otherwise of the New Chemistry were debated. Among these were the translations of foreign texts such as Fourcroy's

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3 Harry Collins, *Changing Order*, 1985, pp. 142–48.

4 See Richard Yeo, *Encyclopaedic Visions: Scientific Dictionaries and Enlightenment Culture*, 2001, especially ch. 10.

5 Thus amidst negotiations with Henry, Lord Brougham, about the wording of the inscription for the Westminster monument Watt Jr was looking at his father's composition of water papers. At one stage he asked Brougham: 'I know not whether it would be proper in the Inscription to allude to this, certainly the most important theory of the later half of the last century … '. See James Watt Jr to Henry Brougham, 27 October 1834, Brougham Papers, 27, 514. In the end no mention was made of this in the inscription.

Elements of Natural History and of Chemistry. In the text itself there is little reference to the 'water question' but, not surprisingly, a very French-centred exposition of the New Chemistry. William Nicholson's 'Preface' to his translation of Fourcroy in 1788 tried to make it clear that while the French systematized the findings concerning the composition and decomposition of water, the British philosophers had been instrumental in generating those findings and had not gained enough credit. Nicholson's main concern was to restore the credit that he considered due to Cavendish. He believed that historical mistakes had been made, caused by 'the want of a speedy and faithful communication of philosophical discoveries between Great Britain and the Continent, together with the unprincipled conduct of such persons as are daily employed in endeavouring to appropriate to themselves the discoveries of others'. Watt came in for mention, much as in Nicholson's *Dictionary of Chemistry* discussed in detail below.[6]

When he came to the specific issue of the discovery of the composition of water, Nicholson began with Macquer's experiment in 1776 in which inflammable air was burned in a bottle and a white china saucer placed over the flame was moistened. The fluid appeared to Macquer to be pure water, but no tests were done. Then Nicholson reported the experiments of Lavoisier and Macquer of 1777, and then those of Warltire before April 1781, and of Priestley.

> It was in the summer of the year 1781, that Mr Henry Cavendish was busied in examining what becomes of the air lost by phlogistication, and made those valuable experiments which were read before the Royal Society on the 15th of January, 1784 … This great philosopher, who may be considered as the true discoverer of the composition of water, appears to think, with Mr Watt, that in those experiments of Dr Priestley's, in which the vitriolic and nitrous acids seemed to be converted into dephlogisticated air, the acids served only to decompose the water by depriving it of its phlogistic part; but he thinks it unnecessary to include the consideration of elementary heat as Mr Watt does, because, in his opinion it is more likely that there is no such thing and that the bringing the consideration forward in every chemical experiment, in which increase or diminution of heat takes place, might occasion more trouble and perplexity than it is worth.[7]

Returning to his chronology, Nicholson noted that Watt interpreted Warltire's and Priestley's experiments differently:

> [He] inferred from these experiments, that water is a compound of the burned airs, which have given out their latent heat of combustion; and communicated his sentiments to Dr. Priestley in a letter, dated April 26, 1783, and he concludes, that in every case wherein dephlogisticated air was produced, water has been decomposed, by the use of some substance which had a stronger attraction to its phlogiston than is possessed by the dephlogisticated air, which is therefore set at liberty. He repeated some experiments particularly with a view to decide this point, and in several of them the quantity of

[6] A.F. Fourcroy, *Elements of Natural History and of Chemistry … Translated into English, with Occasional Notes, and an Historical Preface by the Translator*, 1788, pp. iii–iv. Not the least of Fourcroy's sins was his award of the understanding of heat to Wilcke, Irvine, Crawford, Kirwan, Lavoisier and De la Place. Nicholson's translation added a note: 'Dr. Black of Edinburgh is indubitably the father of the modern doctrine of heat' (p. 116).

[7] Ibid., pp. xi–xii.

> dephlogisticated air added to the acid which came over, greatly exceeded the original weight of acid employed. He dissolved magnesia, calcareous earth, and minium respectively in pale nitrous acid, and on distilling to dryness, found nearly the whole of the nitrous acid in the retort, highly phlogisticated. From common nitre the dephlogisticated air was sixteen times the weight of the nitrous acid which was missing. Mr Watt has therefore a claim to the merit of a discoverer with regard to the composition of water, and has the advantage of priority in the discovery of its decomposition.[8]

Nicholson's account of Watt's work is notable for a number of things. Whilst he gave priority to Cavendish so far as the composition of water was concerned, he did give extensive attention to Watt. He presented Watt as an independent, but not the first, discoverer of the composition of water and gave him priority in discovery of the decomposition of water.

Another translation of the fifth edition of Fourcroy was published in Edinburgh in 1800 with notes by John Thomson, Surgeon.[9] Thomson too was anxious to assert Cavendish's claims against those assumed by Fourcroy for the French philosophers.[10] He did so by pointing to Cavendish's experiments of 1781, to Lavoisier's expectation of the production of an acid rather than pure water on the combustion of hydrogen and oxygen, and to the fact that Lavoisier's famous experiment of June 1783 was professedly indebted to Cavendish's prior work. Watt received a minor mention later in the story: 'It is but justice to add, that the same inference, respecting the decomposition of water, had been made by Mr Watt, and communicated by him in a letter to Dr Priestley, dated April 26 1783.' It is odd that Watt is not credited with *anything* so far as the composition of water is concerned, only with the inference regarding decomposition.

Among a number of important manuals of chemistry published in the nineteenth century was that produced by William Thomas Brande of the Royal Institution.[11] The 'Historical Sketch of the Origin and Progress of Chemical Philosophy' that Brande provided as a Preface to that work traced the origins of chemistry as a science to Boyle, Hooke and Mayow in the seventeenth century. Knight is right when he states that 'the reader would come away with no feeling of an eighteenth-century Chemical Revolution except for the new vocabulary; and with the idea that the honest endeavours of Englishmen and others had been unfairly eclipsed'.[12] Brande dealt with the composition of water in words very close to those deployed in *The London Encyclopaedia* discussed below. Once again we have a publication casually mentioning Watt's inference but focusing upon Cavendish's 'masterly manner'. This statement was to change significantly, especially in the 1841 edition of the work.

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[8] Ibid., pp. xiii–xiv.

[9] A.F. Fourcroy, *Elements of Chemistry and Natural History To Which is Prefixed the Philosophy of Chemistry, 5th edition with notes. By John Thomson, Surgeon, Edinburgh*, 3 vols, 1800. Thomson rose from assistant apothecary at the Edinburgh Royal Infirmary in 1790 to the position of surgeon there a decade later, and Professor of Surgery to the College of Surgeons of Edinburgh in 1805 and the Professorship of Military Surgery a year later at the University of Edinburgh. In 1799–1800 he taught a chemistry class in Edinburgh. His translation of Fourcroy went to five editions (*DNB*).

[10] He does this in note 'q', on pp. 237–44.

[11] William Thomas Brande, *A Manual of Chemistry*, second edn, 3 vols, 1821.

[12] Knight, 'Revolutions in Science', p. 63.

Another writer who gave some attention to Watt was John Murray M.D. (died 1820) whose *Elements of Chemistry* and *System of Chemistry* went through a number of editions. After Murray's death in 1820 these were edited by his son, also, rather confusingly, John Murray. Murray Senior's first statement on the water question came in the first edition of his *Elements of Chemistry*, published in two volumes in 1802. Under the heading 'Water' we find this:

> The idea of the composition of water seems to have occurred about the same time to Mr. Watt and Mr. Cavendish; the former inferring it from some experiments in which it was decomposed; the latter, from finding that when oxygen and hydrogen gases are fired together, water is the only sensible product. The latter experiment was soon after performed on a large scale with the greatest accuracy by Lavoisier and the French chemists, with the same result.[13]

Murray goes on to explain that besides its intrinsic importance, this discovery also had an 'adventitious consequence' at the time in the debate between the supporters and opponents of phlogiston. This attribution is interesting first because it appears to divide the credit equally, even mentioning Watt first. It also, like many of these early attributions, emphasized that Watt's idea came from decomposition experiments rather than from Priestley's composition experiments.

Murray's *A System of Chemistry*, which had reached its fourth edition by 1819, offered a similar, though more extended, story. First we are told that it was concluded 'nearly about the same time, by Watt and Cavendish, that water is a compound, formed from the combination of the base of this inflammable air with oxygen'.[14] Then in a later section we are given a more detailed account. The conclusion, we are told, was drawn 'nearly at the same time by Watt and by Cavendish', their memoirs being published in the same volume of the *Philosophical Transactions*. Watt was now depicted as relying upon Priestley's decomposition *and* composition experiments in drawing his inference. Cavendish was differentiated and elevated somewhat. In his case the compound nature of water 'was established, on evidence still more decisive', and Cavendish sorted out the problem of the production of acid during the combustion of hydrogen.[15] Thus, in this more discursive account in *A System*, Watt was still given considerable credit, and still mentioned before Cavendish, but the latter's efforts were depicted as more decisive and sustained.

Meanwhile *Elements of Chemistry* was going through its own sequence of editions. An advertisement at the front of the fifth edition of 1822 carried the, not quite impartial, statement that the work had 'so long been considered as standard by the Public'.[16] This fifth edition had been revised by John Murray, the younger, and we do find some significant alterations so far as 'water' is concerned that bring Cavendish to the fore. In a move increasingly typical of the genre, 'the facts' were given first:

> When hydrogen gas is burnt, an operation in which oxygen is combined with it, water is formed, and is the only sensible product; and when water is acted on by substances

13 John Murray M.D., *Elements of Chemistry in Two Volumes*, 1802, vol. 1, p. 244.

14 John Murray, *A System of Chemistry*, fourth edn, 1819, p. 98.

15 Ibid., pp. 111–13.

16 Murray, *Elements of Chemistry in Two Volumes*, revised by John Murray, 1822, p. iv.

> capable of attracting oxygen, these are oxidated, the water disappears, and hydrogen gas is evolved. From these facts, the conclusion was drawn by Cavendish and Watt, that water is a compound of hydrogen and oxygen; and the experiment, of forming water by burning hydrogen gas, was executed by the former philosopher on a considerable scale, and its composition clearly demonstrated.[17]

This statement elevated and differentiated Cavendish further. He was now placed before Watt as the first mentioned. Whilst they both drew conclusions from the facts, Cavendish 'clearly demonstrated' the composition. This is also a good example of how texts reinterpreted the work of Cavendish and Watt within the framework of the New Chemistry. Taken literally as an account of what they did, this statement is hopelessly inaccurate, whiggish and positivistic. Yet as a text it undoubtedly works. Murray's texts did carry Watt's claim into the 1820s and beyond and were noted by James Watt Jr and Muirhead to counter the view that the consensus had been entirely for Cavendish. Nevertheless we can detect in these works and their ongoing revisions what we might call an 'attributional drift' in favour of Cavendish.

There were texts in this early period, however, that did not mention Watt at all. Given its lack of scope for extensive exposition, it is perhaps unsurprising that Jane Marcet's *Conversations on Chemistry* was one of these. Yet Mrs Marcet did manage to mention Cavendish. The work was, of course, a very elementary one and it was intended particularly for a female readership. Material was presented in conversational style (being considered as more accessible to women in that form), the conversation occurring between Caroline, Emily and the Governess, Mrs B. Conversation VII is on hydrogen and explores the properties of the gas, including its combustion to form water. Then Mrs B informs the girls:

> The composition of water was demonstrated about the same period, both by Mr. Cavendish, in this country, and by the celebrated French chemist Lavoisier. The latter invented a very perfect and ingenious apparatus to perform, with great accuracy, and upon a larger scale, the formation of water by the combination of oxygen and hydrogen gases.[18]

So Watt has disappeared, Cavendish and Lavoisier do the demonstrating, and Lavoisier provides the large-scale proof. Though clearly not a serious chemical text, Marcet's *Conversations*, which went through numerous editions and was not restricted in use to female students, should not be underestimated for its opinion-making potential among non-specialist audiences.

The same can be said for Samuel Parkes's *The Chemical Catechism*. This, like Marcet's *Conversations*, was first published in 1806 and went through numerous editions. Its dogmatic form of presentation, however, did not lend itself to ready revision and it accumulated in later editions a cumbrous set of notes. In the section 'Of Water', the catechism asks: 'how is it known that water is a compound substance?'

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17 Ibid., p. 305.

18 [Jane Marcet], *Conversations on Chemistry; in which the Elements of that Science are Familiarly Explained and Illustrated by Experiments*, 2 vols, 1817. This was the fifth edition. A reader of the copy that I consulted (held in Special Collections, Edinburgh University Library) had written in the margin at this point, by way of correction: 'After Mr Cavendish had discovered water to be the product Lavoisier <u>enlarged</u> upon it, or extended the expt'.

The answer given is an experimental one, but a note informs us that 'This [the compound nature of water] was discovered by Mr. Cavendish in 1781. Dr. Priestley had previously combined the two gases by combustion; but Mr. Cavendish was the first who drew the proper conclusion from the Doctor's experiment.'[19]

One might be forgiven at this stage for thinking that 'serious' texts tended to mention Watt whilst less serious, or more elementary, ones did not. However, this view cannot be sustained, as the Aikins' *Dictionary of Chemistry and Mineralogy* illustrates. Published in 1807, in two substantial volumes, this was a major work of reference in its time. The compilers explained in their prefatory remarks that they had been forced to leave much out. This included everything relating to geology, to the applications of chemistry to medicine, 'and to the History of science, except to reclaim the merit of discovery in a few disputed instances for those to whom it appeared to be justly due'.[20] If we take this literally to mean that historical material was *only* included when merit was being redistributed, then we have to take the authors' tribute to Cavendish, and omission of any reference to Watt, as an instance of their doing justice in the case of the composition of water.

Under the entry for hydrogen gas the Aikins informed us that 'it is unquestionably owing chiefly to the valuable researches of Mr. Cavendish that the production of water and the combustion of inflammable air have been proved to be constantly concomitant'.[21] In the entry for 'Water', the discussion of the composition of water first mentioned Macquer, Warltire and Priestley before giving details of Cavendish's experiments and telling us that Cavendish drew the conclusion that water was composed of oxygen and hydrogen. There followed an account of the experiments of Lavoisier and Laplace, of Le Fevre de Gineau and of Vauquelin, Fourcroy and Seguin. Finally, Priestley's rearguard action was discussed and dismissed. The Aikins concluded that the experiments on this question of the composition of water 'have been performed with such particular care by the most accurate and intelligent chemists of the age, that no fact in the whole circle of chemical science is more satisfactorily demonstrated'.[22] Clearly, though, in the Aikins' view, Watt had no share in this. Their omission of any mention of him in a detailed discussion, in such a substantial work of reference, could not fail to have some ongoing attributional effects.

Finally, let us examine one of the most important chemical works of the early nineteenth century, Humphry Davy's *Elements of Chemical Philosophy*, published in 1812. The work begins with a historical view of the progress of chemistry. In this Cavendish loomed large alongside Lavoisier. Except for Cavendish, Davy declared, there was no one who could be compared with Lavoisier 'for precision of logic, extent of view, and sagacity of induction'. Cavendish himself was noted for his wide and minute knowledge of Natural Philosophy, and

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[19] Samuel Parkes, *The Chemical Catechism*, eighteenth edn, 1818. On Parkes's and Marcet's works see David Knight, 'Accomplishment or dogma: Chemistry in the introductory works of Jane Marcet and Samuel Parkes', *Ambix*, **33**, 1986, 94–98.

[20] A. and C.R. Aikin, *A Dictionary of Chemistry and Mineralogy*, 2 vols, 1807, p. vi.

[21] Ibid., vol. 1, p. 556.

[22] Ibid., vol. 2, p. 474. The article 'Water' covers pages 470–86 and the discussion of the composition of water is on pp. 472–74.

> he carried into his chemical researches a delicacy and precision, which have never been exceeded: possessing depth and extent of mathematical knowledge, he reasoned with the caution of a geometer upon the results of his experiments: it may be said of him, what, perhaps, can scarcely be said of any other person, that whatever he accomplished, was perfect at the moment of production. His processes were all of a finished nature; executed by the hand of a master, they required no correction; the accuracy and beauty of his earliest labours even, have remained unimpaired amidst the progress of discovery, and their merits have been illustrated by discussion, and exalted by time.[23]

Davy noted that Cavendish continued to adhere to the doctrine of phlogiston. However, the implication was that his masterly works transcended adherence to that erroneous philosophy, so that the doctrine of Lavoisier 'soon after it was framed, received some important confirmations from the two grand discoveries of Cavendish, respecting the composition of water, and nitric acid'.[24] Davy in this way assimilated Cavendish to the New Chemistry of Lavoisier, attributed to him the discovery of the composition of water, and made no mention whatever of James Watt.[25] There was a real sense in which Davy's painstaking experiments in the electrolysis of water were his tribute to Cavendish's style and precision.[26] Within the chemical community itself, and among the wide circle of Davy's fashionable admirers garnered through his Royal Institution lectures, the treatment of the water question in this influential work would have been an important resource. We will see that Watt Jr was to take some trouble in the 1820s to try to change Davy's mind.

Dictionaries and Encyclopaedias

The 1797 third edition of the *Encyclopaedia Britannica* included in the article 'Water' an extended 'Historical Account of the Discovery of the Composition of Water',[27] followed by a description of a series of experiments presented as 'Proofs of the Composition of Water'. The historical section acknowledged immediately that there was an issue of attribution:

> The history of this curious and interesting discovery we shall trace back with as much precision and impartiality as possible to the first hints that were thrown out upon the subject, and endeavour at the same time to assign to all who have contributed to the discovery the merit to which they are respectively intitled.[28]

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[23] Humphry Davy, *Elements of Chemical Philosophy*, 1812, pp. 37, 42. It is hard not to see in these remarks, also, a comparison between the durability of Cavendish's work and the undermining of aspects of Lavoisier's that Davy was engaged in.

[24] Ibid., pp. 43–44.

[25] Watt is mentioned in Davy's *Elements* but solely in relation to his investigations of latent heat and their connection with his improvements of the steam engine (pp. 86–87).

[26] Partington, *History of Chemistry*, vol. 4, 1964, pp. 41–42 and Colin A. Russell, 'The electrochemical theory of Sir Humphry Davy', *Annals of Science*, **15**, 1959, 1–25; **19**, 1963, 255–71.

[27] *Encyclopaedia Britannica*, third edition, vol. 18, 1797, pp. 807–808.

[28] Ibid., p. 807.

Warltire's work of 1781 was recounted, as was Cavendish's experimental work of the same year which was represented as a repetition of Warltire's experiment, occurring after the publication of Priestley's account of Warltire's work in the fifth volume of his *Experiments and Observations* in 1781. We are told, following Cavendish's own account in the *Philosophical Transactions* for 1784, that 'Mr Cavendish concluded, that when inflammable and common air are exploded in a proper proportion, almost all the inflammable air, and near one-fifth of the common air, lose their elasticity, and are condensed into dew; which, when examined is found to be water'.[29] Dealt with next were Priestley's experiments that were undertaken after Cavendish had informed the Reverend of his experiments. What Priestley did, and why he did it, was expressed in an interesting fashion:

> Having formerly observed several remarkable changes in fluid substances, in consequence of long exposure to heat in glass vessels hermetically sealed, Dr Priestley formed a design of exposing all kinds of solid substances to great heats in close vessels. As many substances consist of parts so volatile as to fly off before attaining any considerable degree of heat in the usual pressure of the atmosphere, he imagined that if the same substances were compelled to bear great heats under a greater pressure, they might assume new forms, and undergo remarkable changes.[30]

These, then, were the experiments that were drawn to Watt's attention and stimulated his exertions on the composition of water question. It is noteworthy that both Priestley's and Watt's trajectory into that question focused upon the role of heat in relation to major changes in form. In Watt's case he was clearly still thinking very much within the tradition established (with Watt's help) twenty years earlier by his friend Joseph Black. Watt's initial response to Priestley was to mention 'a similar idea of his, that it might be possible to convert water or steam into permanent air'. The author of the article then reports from Watt's account of his theory as published in the *Philosophical Transactions* in 1784. The centrality of heat is once again apparent. Watt had long held the belief that 'air was a modification of water'. This belief, we are told, 'arose from a discovery; that the latent heat contained in steam diminished in proportion as the sensible heat of the water from which it was produced increased. In other words the denser the steam was, the less latent heat it contained.' Watt is quoted at length on the deflagration of inflammable and dephlogisticated air:

> These two kinds of air unite with violence, they become red hot, and upon cooling totally disappear. When the vessel is cooled, a quantity of water is found in it equal to the weight of the air employed. The water is then the only remaining product of the process; and water, light, and heat are all the products … Are we then not authorised to conclude; that water is composed of dephlogisticated air and phlogiston deprived of part of their latent or elementary heat; that dephlogisticated or pure air is composed of water deprived of its phlogiston and united to elementary heat and light; and that the latter are contained in it in a latent state, so as not to be sensible to the thermometer or to the eye; and if light be only a modification of heat, or a circumstance attending it, or a component part of the

29 Ibid.

30 Ibid.

> inflammable air, then pure or dephlogisticated air is composed of water deprived of its phlogiston and united to elementary heat.[31]

The author of the article 'Water' had already expressed the view that Watt's theory had been demonstrated to be true. Returning to this, he anticipated the objection that some would deny this because Watt employed the term 'phlogiston', 'a word which is now exploded from philosophy as the name of an imaginary substance'. The author met this anticipated objection by observing that Watt used the word 'as synonymous with inflammable air'.

What I find particularly interesting and revealing about the quotation from Watt, the anticipated difficulty, and the response to that difficulty, is that Watt's treatment of the role of heat and light in the process was not considered remarkable or problematic. It was noted that Cavendish continued to differ from Watt on the role of heat since Cavendish did not believe that there was such a thing as elementary heat. However, this was not seen as a problem in accrediting Watt's theory. The presentation was quite comfortable in leaving Watt's theory firmly, if implicitly, in the Black chemical tradition. Thus the chemical/material nature of heat, or otherwise, was not selected as a major issue in evaluating the worth of theories.

The section concluded with an allocation of credit in the discovery. Watt 'was the first person who formed the true theory'. Importantly, however, his theory was not a speculation quickly perceived in a eureka-like experience. Watt had suspected something of the sort for many years, believing that 'if the latent heat of steam could be wholly converted into sensible heat by a great increase of heat, the steam might suffer some remarkable change, such as into permanent air'.[32] The author made the point that Watt was prepared and primed, this being evident from the speed with which he formed the theory on hearing of the experiment from Priestley. Cavendish, independently of Watt, made ingenious experiments, 'which led him to conclude, that it was highly probable that water was a composition of air'. Cavendish 'went as far as his experiments would permit him, and he went no further'. The import of this remark was that Cavendish, independently of Watt but subsequent to him, formed the theory and did so on a strong experimental basis of his own, cautious, making. The author treated Priestley as being merely the 'instrument of promoting this discovery'. Priestley was attacked for the vagueness of his ideas and the randomness of his experiments, and his continuing opposition to Lavoisier's chemistry was seen as irrational. The author (almost certainly John Robison) expressed himself 'now no longer surprised at the singularity of Dr Priestley's opinions in religion; either at his incredulity in some high things, or at his licentious sentiments in others'.[33] Black was then mentioned, by contrast, as doing the honourable thing in candidly admitting in his lectures the truth of the Lavoisian System. Lavoisier, to his great credit, 'was the first person who demonstrated the theory, and put it beyond doubt'.

So far as our immediate focus is concerned, then, the third edition of the *Britannica* in its explicit treatment of the history of the theory of the composition of water

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31 Ibid., p. 808, quoting from Watt, 'Thoughts on the constituent parts of water', p. 333.

32 *Encyclopaedia Britannica*, third edn, vol. 18, p. 808.

33 Ibid., p. 809. This will be recognized, so far as Priestley is concerned, as a typical 'positivist-whig' move to consign Priestley to the sphere of darkness.

offered a nuanced account in which credit was divided in various ways on a variety of criteria. There is no doubt, however, that Watt was attributed with the first statement of the true theory. The *Britannica*, however, was not consistent in its treatment of the issue. Where nuance was not possible, or redundant, Cavendish was credited. Thus in the article 'Steam', amidst further recountings pertaining to material heat and light, we find the bald reference to Cavendish as the discoverer of the chemical composition of water.[34] It seems to me that Cavendish would not recognize his discovery in the account of it given in this article. The account smacks so much of the Black chemical tradition that it is a peculiar attribution indeed.[35]

After the 1797 edition of the *Britannica* the next major vehicle in which material relevant to the water question appeared was the 1801 *Supplement* to the third edition, produced under the editorship of George Gleig. A major contribution to volume one of this *Supplement* was a new article on 'Chemistry' by Thomas Thomson. Thomson noted that, although the article 'Chemistry' in the third edition was written 'only about ten years ago, the language and reasoning of chemistry have been so greatly improved, and the number of facts have accumulated so much, that we find ourselves under the necessity of tracing over again the very elements of the science'.[36] Thomson, in effect, weeded out what he regarded as the archaisms of his predecessor. In the section dealing with the composition of water, Thomson noted that this great discovery 'has contributed more perhaps than any other to the advancement of the science of chemistry, by furnishing a key for the explanation of a prodigious number of phenomena'. His historical account mentioned Scheele (as making the first attempt to discover what was produced in the burning of hydrogen gas), Macquer (1776), Bucquet and Lavoisier (1777). Warltire's experiments of 1781 with Priestley's encouragement and participation are then discussed. We are next told quite straightforwardly that Cavendish, from his experiments conducted in 1781, 'concluded that water was a compound – Mr Cavendish must therefore be considered as the real discoverer of the composition of water'.

> He [Cavendish] was the first who ascertained that water was produced by firing oxygen and hydrogen gas, and the first that drew the proper conclusion from that fact. Mr Watt, indeed, had also drawn the proper conclusion from the experiments of Dr Priestley and Mr Warltire, and had even performed a number of experiments himself to ascertain that fact, before Mr Cavendish had communicated this; but he [Watt] had been deterred from publishing his theory by some experiments of Dr. Priestley which appeared contrary to it. He has therefore a claim to the merit of the discovery; a claim, however, which does

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[34] 'Steam', *Encyclopaedia Britannica*, third edn, 1797, vol. 18, pp. 733–43, at p. 737. The relevant section states: 'in vital or atmospheric air there is not only a prodigious quantity of fire which is not in the vapour of water, but … it also contains light, or the cause of light, in a combined state. This is fully evinced by the great discovery of Mr Cavendish of the composition of water. Here we are taught that water (and consequently its vapour) consists of air from which the light and greatest part of the fire have been separated.'

[35] Those places in Black's lectures, as edited by Robison, where Cavendish is credited with the discovery were gladly noted by the Cavendish camp or 'kept quiet' by the Watt supporters.

[36] 'Chemistry', *Supplement to the Encyclopaedia Britannica*, 1801, 2 vols, vol. 1, p. 212.

> not affect Mr Cavendish, who knew nothing of the theory and experiments of that ingenious philosopher.[37]

The primary credit was given to Cavendish, but Watt was also admitted to have 'a claim'. That claim was treated, however, as quite independent of Cavendish's. The suggestion appears to be that the discoveries were quite separate and equivalent, though Cavendish was treated as the incumbent and Watt as the 'other claimant'. The account proceeded to a discussion of Lavoisier's work and hence to 'proofs' of the composition.

The 1810, fourth, edition of the *Britannica* retained in many respects the article 'Chemistry' as set out by Thomson. However, the section dealing with the composition of water had undergone a crucial change.[38] Significantly, I think, the 'Proof of the Composition of Water' was now presented *before* the 'History of the Discovery'. This was part of a general tendency to supersede historical by analytical accounts of the science. Rather than the chief facts of the science being conveyed historically, they were now presented in a more analytical or demonstrative fashion. The proofs consisted largely of a recounting of Lavoisier's experiments. It was then observed that, while Lavoisier provided the final confirmation of the component parts of water, that knowledge was 'indebted for its origin and progress, chiefly, if not entirely to the English philosophers'.[39]

The brief historical account is presented in a way less concerned with, and less certain as to, priority. Of Cavendish's experiments as made in 1781 Thomson says they were 'undoubtedly conclusive with regard to the composition of water'. Watt still featured; he was described as appearing to 'entertain the same ideas' on the subject. The quotation from the key letter to Priestley was given and Thomson commented: 'Thus it appears that Mr Watt had a just view of the composition of water, and of the nature of the process by which its component parts pass to a liquid state from that of an elastic fluid.'[40] Thomson's focus, more so than in 1801, was upon how the *fact* was eventually established beyond doubt. Thomson's account, as we have seen, appeared in a number of publications from the 1801 supplement to the *Britannica* onwards, including his *History of the Royal Society*, which still mentioned Watt. By 1830 in his *History of Chemistry*, short, rather pat accounts were no longer mentioning Watt and making unproblematic attributions to Cavendish. Thomson's 'line' on such matters had become very clearly identifiable to Watt Jr and Muirhead, who were in the habit in the 1830s and 1840s of referring to the 'Thomsonian' approach to the water question.[41]

Returning to the *Britannica*, we note that from the *Supplement* to the fourth, fifth and sixth editions produced from 1815 onwards under the editorship of Macvey Napier, a systematic attempt was made to include biographical accounts of major figures. Thus it was that articles on Cavendish and on Watt were added to those on

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[37] Ibid., p. 289.

[38] 'Chemistry', *Encyclopaedia Britannica*, fourth edition, 1810, vol. 5, pp. 496–98.

[39] Ibid., p. 497.

[40] Ibid., p. 498.

[41] See, for example, Watt to Muirhead, 10 January 1840, Muirhead Papers, MS GEN 1354/463. Thomas Thomson, *History of the Royal Society*, 1812, p. 471.

'Chemistry' and 'Water' as places where the composition of water might be discussed. The article on Cavendish was contributed by Thomas Young, that on Watt by James Watt Jr, albeit anonymously.[42]

Other encyclopaedias often followed the same basic pattern as had emerged in the later editions of the *Britannica,* mentioning Watt, giving him some credit, but attributing the discovery to Cavendish. The *Encyclopaedia Edinensis*, a compact encyclopaedia in six volumes intended for a popular audience, contrived to do this by awarding Watt the discovery in the biographical article on the great engineer (an article that appears heavily reliant on Watt Jr's memoir in the *Britannica*) but awarding it to Cavendish in the discussion of water in the article 'Chemistry'. Consistency was not always a strong point in encyclopaedias and was probably not helped in the case of the *Edinensis* by the death of the original editor during the course of its production.[43]

The London Encyclopaedia, published in 1829 in twenty-two volumes, described itself as offering a 'popular view of the present state of knowledge'. It dealt in various ways with the composition of water in its entries for 'Water' and for 'Chemistry'. Under 'Water' we are told that Cavendish

> demonstrated about the year 1784, that [water] is composed, in fact, of two distinct aeriform fluids – oxygen and hydrogen; containing eighty-eight parts by weight out of every 100 of the former and twelve of the latter. This was the result of three years of laborious experiments.
>
> The late Mr. Watt, the celebrated improver of the steam-engine, seems to have inferred at the same period (in 1783), independently of Mr Cavendish, that water was a compound body of the kind it has been since proven to be … [44]

Later in the same article, in reference to oxygen it was stated that 'It was reserved for Mr. Cavendish, as we have seen, first to suggest in 1781, and prove beyond dispute in 1784, the important quantum of this vivifying ingredient contained in water.'[45] Once economy of expression was required, the modalities concerning others' contributions were dropped and the 'fact' of Cavendish's discovery simply stated.

The article 'Chemistry' in *The London Encyclopaedia* contained an intermediate statement mentioning both Watt and Cavendish but concentrating upon the latter. We are told that Cavendish's experiments led to the discovery of the composition of water. The production of dew on the combustion of hydrogen had been noticed by a number of people. It was 'referred by Mr. Watt to the production of water in 1783;

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[42] These are examined below, and the memoir of Watt in the *Britannica* is discussed in detail in Chapter 5.

[43] See *Encyclopaedia Edinensis: or Dictionary of Arts, Sciences and Literature in Six Volumes*, 1827, 'Chemistry', vol. 2, pp. 255–387, at p. 276, 'Watt, James', vol. 6, pp. 638–39, at p. 638. The original editor was Dr James Millar, who survived long enough to write the article 'Chemistry' but not that on 'Watt'.

[44] 'Water' in *The London Encyclopaedia or Universal Dictionary of Science, Art, Literature, and Practical Mechanics, comprising a Popular View of the Present State of Knowledge*, 1829, 22 vols, vol. 22, pp. 565–69, at p. 567.

[45] Ibid., p. 568.

but experimental proofs were still wanting, and they were supplied in a masterly manner by Cavendish in a paper given to the Royal Society in 1784'.[46] Throughout these accounts, then, a consistent impression was given of a qualitative difference between the two researchers. Little emphasis was placed on dates. Indeed, in the 'Chemistry' article, to judge by dates alone, Watt would seem to have priority. But Cavendish's 'masterly manner' was emphasized, in a way reminiscent of Davy's depiction. Generally, whilst Watt 'infers', Cavendish 'proves' and 'demonstrates'. Given the similarities between these articles and the account given in Brande's *Manual of Chemistry*, it appears that Brande was the author. A similar recycling process occurred with the writings of William Nicholson.

As we have seen, Nicholson's first statement was made in his translation of Fourcroy's *Elements of Natural History and of Chemistry*, published in 1788.[47] One of the key objectives of his historical preface was the restoration of credit to Cavendish in the face of French claims. A very similar account was given in Nicholson's *A Dictionary of Chemistry*, published in two volumes in 1795. The most extensive discussion of the composition of water in this work occurred in the article 'Water'.[48] Nicholson made some observations there on the issue of discovery:

> The powers of nature, which are ever the same, and are continually performing their operations before us, whether we understand them or not, often present facts of the utmost value and importance, which we overlook, or regard with indifference. Hence it happens, that when an enlightened observer makes any discovery, it is almost always observed that somebody has seen the fact before him, or given some confused hints respecting its theory. It is evident, however, that the first discoverer, if there be any merit in discovery, is not the man who finds the treasure, and supposes it to be none, but he who is conscious of its value, and applies it to use. On these principles it is, that the claims of the discoverers of the composition of water must be estimated.[49]

Nicholson then presented the 'facts' much as he had done in his translation of Fourcroy. The exploits of Macquer, Bucquet and Lavoisier, Warltire and Priestley were recounted but no 'award' of the title of discoverer made. Cavendish, however, on the strength of his 1781 experiments and the conclusions drawn from them 'may be considered as the true discoverer of the composition of water'.[50] Watt's inferences from Warltire's and Priestley's experiments and his own experiments, to test the proposition that wherever dephlogisticated air is produced water has been decomposed, were also regarded as highly significant. We are told, as we were in Nicholson's edition of Fourcroy, that 'Mr. Watt has therefore a claim to the merit of

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46 'Chemistry', *The London Encyclopaedia*, 1829, vol. 5, pp. 363–560, at p. 369.

47 A.F. Fourcroy, *Elements of Natural History, and of Chemistry; being the second edition of The Elementary Lectures on those Sciences, first published in 1782. Translated into English. With occasional Notes and an Historical Preface, by the Translator*, 4 vols, 1788. Nicholson's important text, *The First Principles of Chemistry*, 1790 descried the system-building of Lavoisier and the speculations of Priestley.

48 'Water', in William Nicholson, *A Dictionary of Chemistry*, 2 vols, 1795, vol. 2, pp. 1013–25.

49 Ibid., p. 1018.

50 Ibid., p. 1019.

a discoverer with regard to the composition of water, and has the advantage of priority in the discovery of the decomposition'.

So Nicholson called Cavendish the '*true* discoverer' while describing Watt as '*a* discoverer'. We are left to guess how he saw this distinction following from the principle of attribution that he had enunciated in the beginning. Perhaps Watt was close enough to the 'right answer' (apart from the business about latent heat) to be regarded as 'conscious of the value' of the inferences made from experiments. Operating independently of Cavendish, he was *a* discoverer. Cavendish, though, in getting the 'right' answer and proving it by quantitative experiments, was to be regarded as the *true* discoverer. He, more than anyone, had, according to Nicholson's dictum, found the treasure, was conscious of its value and applied it to use.

Nicholson's text had an interesting fate in the hands of Andrew Ure, who took over the *Dictionary of Chemistry* as it was published in 1821.[51] The entry for 'Water' in Ure's publication was taken, as was much else, largely verbatim, from Nicholson's *Dictionary*. However, there were two crucial differences. First, at the end of the passage dealing with Cavendish's experiments, the sentence describing him as the 'true discoverer' was simply omitted. Although the section providing details of Watt's experiments and describing him as '*a* discoverer' was also omitted, the key words (about Watt inferring that water is a compound) remain.[52] Reading Ure's *Dictionary* one might well conclude that Watt was the first to infer anything about the compound nature of water. Two minor changes transformed a clearly pro-Cavendish statement into a clearly pro-Watt one. In this case we have the word of at least one reader that such was the impression given, the reader being none other than James Watt Jr. In a letter to James Patrick Muirhead, one of a number that discussed the merits or otherwise of various texts relating to his father, Watt Jr expressed his liking for Ure's account: 'Ure's Dictionary of Chemistry, published in 1820, article "Water" … gives my father all I claim for him, and probably was written before my short life of him was printed in the Encyc. Brit., and is on that account the more valuable.'[53] Exactly why Ure took these steps is not known. Ure's association with the Andersonian Institution, his concern with the practical side of chemistry and his strained relations with Thomas Thomson may all have contributed to his decision to give greater prominence to Watt as discoverer in this rather surreptitious manner.[54]

51 Andrew Ure, *Dictionary of Chemistry on the Basis of Mr Nicholson's*, 1821.

52 Ure, *Dictionary of Chemistry*, 'Water', cols 3 and 4 (no pagination).

53 James Watt Jr to J.P. Muirhead, 9 April 1843, Muirhead Papers, MS GEN 1354/643. Watt was actually comparing Ure's account favourably with that given in W.T. Brande's *Manual of Chemistry* at this point. Elsewhere he commented unfavourably on Nicholson. Watt Jr seems not to have realized that Ure's account derived from that of Nicholson. Ure's *Dictionary* was subsequently taken over by Henry Watts's *Dictionary of Chemistry*, which developed yet another interpretation of the water question. See Chapter 11.

54 On Ure see W.V. Farrar, 'Andrew Ure F.R.S., and the philosophy of manufactures', *Notes and Records of the Royal Society of London*, **27**, 1973, 299–324.

Lecture Courses

Lecturing on chemistry became big business from the late eighteenth and early nineteenth centuries. The subject itself became very popular in fashionable circles, as evident, for example, in the lectures of Humphry Davy at the Royal Institution. After the passage of the Apothecaries Act in 1815, chemistry lecture courses proliferated to cater to those seeking to qualify for the medical profession. However, the most important and popular figures in the chemical lecturing stakes were probably the chemists of the Scottish universities, men who passed thousands of students through their lecture halls. Joseph Black, first at Glasgow and then at Edinburgh, was probably the most prominent of these. Doyle calculates that in the 1790s the average attendance at Black's lectures was about 225 and suggests that at Edinburgh from 1766 Black taught about 5000 students. These included numerous individuals subsequently influential in science and medicine. This made Black a significant force in the diffusion of chemical knowledge in the late eighteenth century.[55] The lectures of Black, and his successor as Professor of Chemistry at Edinburgh, Hope, are worth particular attention for two reasons: they are well documented, and they are the place where one would expect Watt to be given his due if he was to be given it anywhere. Here, too, however, we find the credit given to him strictly qualified and quick to fade in the early nineteenth century.

So far as Black's students were concerned, Watt would be known as a chemical collaborator of their great teacher in his research on steam, latent heat and the phenomena of heat more generally. Robison, in his edition of Black's lectures, observed that Watt was from his earliest acquaintance with Black at Glasgow 'a philosopher, in the most exalted sense of the word'. Watt's contributions to Black's researches were 'always recited in the class, with the most cordial acknowledgment of obligation to Mr. Watt'.[56] Black's discussion of the discovery of the composition of water, as rendered in the published version of his lectures, recounted the familiar story of Watt's letter to De Luc of April 1783 on Priestley's experiments, the decision to delay publication of that letter, Cavendish's renewed attention to the problem and his publication of results and conclusions already communicated to Lavoisier.

> although Mr Cavendish is the undoubted author of this decisive experiment, and, with Mr. Watt, is also the author of the important doctrine of the composition of water, Mr Lavoisier has the still greater merit of seeing this proposition *in all its importance. This* incited him to undertake these laborious and expensive experiments, which confirmed those of Mr Cavendish beyond a doubt; and he also had the sagacity to perceive *immediately*, that by means of this proposition, he should extricate his great system from difficulties and objections which I think would otherwise have been unsurmountable … [57]

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55 W.P. Doyle, 'Black, Hope and Lavoisier', in A.D.C. Simpson (ed.), *Joseph Black 1728–1799. A Commemorative Symposium*, 1982, p. 43.

56 *Lectures on the Elements of Chemistry delivered in the University of Edinburgh by the late Joseph Black, M.D … Now Published from his Manuscripts by John Robison, LLD*, 2 vols, 1803, vol. 1, pp. xliii–xliv.

57 Ibid., vol. 2, pp. 237–38.

Elsewhere in the lectures, in a more general statement of the generation of the New Chemistry, Black stated that:

> The fundamental experiments were first made, and the leading inferences were first drawn in this country, by Dr Priestley, the Honourable Mr Cavendish, and my friend Mr Watt. But it was chiefly in France that they were repeated, with proper attention to all the circumstances that would affect the result, and this result was made the foundation of a new theory of combustion.[58]

Black was here giving Watt a share with Cavendish in the discovery of the 'doctrine' of the composition of water, seeming to distinguish the discovery of the doctrine from the experimental work that made it possible, as also from the larger theoretical structure that Lavoisier incorporated it into. However, in other parts of the lectures, and in Robison's commentary thereon, different statements appear that baldly attribute the discovery of the composition of water to Cavendish.[59] To read those alone would give a very different impression of the situation. Such statements may not have been intended to exclude Watt, merely doing so by way of a necessary shorthand in discussing such matters.

As Perrin has argued,[60] Black's role in the reception of Lavoisier's Chemistry in Edinburgh was that of a reluctant catalyst. Black early acknowledged Lavoisier's System as superior and yet he had his doubts about the finality of 'the System'. An examination of students' notes of Black's (and Hope's) lectures suggests the possibility that Black had difficulty in incorporating the New Chemistry into his lecture course. Notes survive that were made by a James Scott in the 1785–86 lecture season, when Black was in regular correspondence with Watt and others about the work on the composition of water. However, at those points in the lectures where these findings would logically have been discussed, there is silence.[61] Lecture notes for 1788 indicate that Lecture 69 on 'Inflammable Bodies'

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58 Ibid., vol. 1, p. 238.

59 See, for example, vol. 1, p. lvii, vol. 2, p. 215, vol. 2, p. 737, note 16. It should be remembered that Robison's contributions were coloured very much by his political sentiments. He mistrusted the way that the French, as he saw it, had operated in their chemistry, that is, in concert, authoritatively, with government endorsement and support, to secure the new chemistry as *French* chemistry. Robison's prime concern was to reclaim credit for Cavendish, not to arbitrate between the British claimants. For Robison's sentiments, see vol. 2, p. 217.

60 C.E. Perrin, 'A reluctant catalyst: Joseph Black and the Edinburgh reception of Lavoisier's Chemistry', *Ambix*, **21**, 1982, 141–76.

61 James Scott, 'Dr Black's Chemestry [*sic*]', 6 vols, Royal College of Physicians, Edinburgh, MS Black 2 (1)–(6). The notes for Lecture 108, in vol. 6, indicate Black saying, before discussing the physical properties of water, that he would not examine there its chemical properties because 'we considered several when speaking of those bodies of w[c] it is the natural menstruum, as the salts and many compounds resulting from the Union of other Bodies w[th] salts as the Metals, Absorbent Earths and Inflammable Substances'. However, when one turns to the previous lectures dealing with those topics mentioned, there is virtually nothing said about water, or experiments relating to its composition. For example, Lecture 69, on Inflammable Bodies, apparently discussed only sulphur, charcoal, ardent spirits, oils and bitumens, there being no sign of 'Inflammable Air'. It is possible that Black regarded these issues as so fluid, as it were, at the time that he decided that the best policy was to leave the material out altogether. A systematic survey of extant notes on Black's lectures has not been attempted

did now discuss inflammable air. Cavendish's work with it was mentioned, but the key section on the explosion of inflammable air with common air or dephlogisticated air mentioned only Priestley: 'Dr Priestley on mixing the two airs in a close vial, and firing them with the Electric Spark, found that they were both consum'd, nothing remaining but a quantity of water equal in weight to the two airs … '.[62]

By the mid-1790s, when Black and Hope were lecturing jointly, lecture notes tell a fuller story:

> Dr. Priestley who first tried this experiment [the explosion of hydrogen mixed with oxygen, now so called] having the airs mixed in the same vessels called them his pocket pistols. It was a question of much difficulty to determine what became of the Airs, but it is now proved that the result is Water for this very important & wonderful discovery we are indebted to Mr. Cavendish. Mr. Lavoisier supposed there must be some product like the Sulphuric Acid or the Carbonic Acid but could come to no conclusion on making experiments. He also observed as well as others that the sides of the vessels had dewy drops.
> Mr. Cavendish previously passed the different Airs through tubes that should be sure to absorb the moisture, he then exploded them & found that when the due portions were used the result was invariably water perfectly pure … This was soon afterwards confirmed by the most superb experiments by the French Chemists.[63]

The only mention of Watt in these lectures is as the inventor of a 'convenient portable' apparatus for the production of hydrogen by the decomposition of water by hot iron. He is not mentioned as a contender for the credit of discovery.

The evidence from lecture notes, while ambiguous in helping us trace what Black was saying to his students, does strongly suggest that, even among his Edinburgh friends, Watt's claims to credit were scarcely mentioned whereas those of Cavendish were repeatedly noted. The same pattern emerges from an examination of relevant papers delivered to the Chemical Society founded by Black's students in 1785–86 in Edinburgh and to the longer-lived Natural History Society.[64] A number of dissertations read before the Chemical Society in 1785 dealt with the composition of water and most attributions were to Cavendish, though there are hints that Black and some of the students adhered to Watt's account involving heat without mentioning

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but rather a sampling made. For details of resources see: William A. Cole, 'Manuscripts of Joseph Black's Lectures on Chemistry', in Simpson (ed.), *Joseph Black 1728–1799*. pp. 53–69.

62 Anonymous, 'Lecture notes on Joseph Black's Lectures, ca 1788', Edinburgh University Library, Special Collections MS Dc.2.42–43, vol. 2, p. 380. The other mention of the composition of water occurs in the lecture, not numbered, in which analysis of waters was discussed. There we find: 'Lavoisier and some French Chemists suppose that water is composed of Empyreal [another name for Dephlog[d]] & Inflammable Air; because upon firing these two airs nothing remains but a small quantity of Water, into which the two airs are changed; and from this Doctrine they are enabled to Explain many phenomena in Chemistry' (vol. 3, p. 320).

63 Anon., 'Chemistry by Drs Black and Hope', 1796–97, Edinburgh University Library, Special Collections, MS Gen 48D, Section 81.

64 On these societies see Perrin, 'A Reluctant catalyst', 141–76 and J. Kendall, 'The first chemical society, the first chemical journal and the Chemical Revolution', *Proceedings of the Royal Society of Edinburgh*, **63A**, 1952, 346–58.

Watt's name.[65] Two dissertations are particularly interesting. One by Thomas Beddoes, titled 'An Attempt to Point out some of the Consequences which Flow from Mr. Cavendish's Discovery of the Component Parts of Water', makes the attribution to Cavendish as the title suggests, but also notes that Cavendish's experiments might be explained another way:

> Dr Black seemed to me to hint at such an explanation & undoubtedly if two different hypothesis [*sic*] can be adjusted to any set of appearances, both become uncertain … Might not the water have been combined with the Airs & so have existed in its proper form[?] If so, it follows that Inflammable & Vital Air differ only in the proportion of their ingredients …

A second essay, by Mr George Kirkaldie, 'On Dephlogisticated Air', raised the issue of whether earth entered into the composition of dephlogisticated air and pointed to Cavendish's experiments as showing that this is not so: 'for from these it appears, that by uniting with a certain quantity of the principle of Inflammability it may be wholly converted into dew, & surely if it contained any Earth, it would be evident upon such condensation'. Kirkaldie also believed that Cavendish's experiments on water were open to a different interpretation. He argued that:

> something else is necessary to the formation of water from Dephlogisticated & Inflammable Air or pure phlogiston, than their simple mixture, & such I am very much inclined to believe is a quantity of Heat supplied by the Electricity, passing into the Latent state during the formation of the water … My idea of Dephlogisticated Air then is, that it consists of a watery principal [*sic*] combined with heat & that by the addition of phlogiston to it, agreeable to the opinion of Mr Cavendish, water is formed, which again being deprived of its heat, is converted into Ice. Dephlogisticated Air then seems only to differ from Vapour, in being destitute of phlogiston, as seems Evident f[ro]m the Specific gravity of these two fluids.

These sorts of objections to Cavendish's interpretation of his experiments, coming from within the Black tradition, echo the explanations that Watt himself was offering of the phenomena. Yet, remarkably and mysteriously, these students made no mention of Watt.

We can only guess, in the end, what Black's students took away from his lectures on the specific question of the composition of water. It does appear, however, that Cavendish was treated as the central figure in the work and that the accurate determinations of the French were also acknowledged. The students would undoubtedly have had a concrete impression of Watt as an active and important chemist from the 1760s onwards and that he did 'hover around' the water question. It does appear that Robison's rendition of these affairs in his edited version of Black's lectures and in the *Britannica* gave a more prominent role to Watt than was perceived in Black's and Hope's lectures.

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65 'Dissertations Read before the Chemical Society Instituted in the beginning of the Year 1785', Edinburgh University Library, Special Collections, MS 2748. There are also a number of interesting papers which discuss the composition of water in 'Papers of the Natural History Society', 15 vols, Edinburgh University Library, Special Collections, Da67NAT (See vol. 2, p. 185ff, vol. 3, p. 180ff, vol. 4, p. 186ff).

The fact that Watt was held out to students as Black's chemical collaborator is perhaps more significant than it might appear. In Black's case the relation with students was regarded as very important. It is well known that Black was very tardy in laying public claim through publication to his own discoveries on heat, in fact he never did so. While there were undoubtedly other reasons for this (Robison describes him as hating authorship), one reason given was the belief that expounding one's discoveries to students was a legitimate, perhaps even sufficient, form of publication.[66] Given the number and quality of Black's students over the years and their ubiquity in British scientific life, this belief probably had some substance. From our immediate point of view it is important to note that what was said about the composition of water in these lectures probably stuck with the auditors and shaped their view of the question in the longer term. Yet Black and his friends did recognize the precariousness of claims based on having expounded discoveries to classes of students. Watt and Robison, among a number of Black's close associates, urged him to publish, recognizing that reputation beyond one's face-to-face circle depended on that.

We must remember, then, that in the late eighteenth and early nineteenth centuries the handling of intellectual property rights was still an ambiguous matter about which many were ambivalent. It was into this climate that Watt's claims regarding the composition of water were launched. It was also in this climate that his decision about whether or not to press his claims (beyond the act of publication in the *Philosophical Transactions*) had to be made. In expressing to his son the view that posterity would decide, he perhaps had in mind not only subsequent readers of the literature but also informal media of communication such as those involving teachers and students.

The successor of Black and Robison in teaching chemistry at Edinburgh University, Thomas Charles Hope taught for Black as the latter's health began to fail in the later 1790s, and continued to do so as Black's successor into the 1840s. His extensive lecture notes, though difficult in many ways to decipher and to date, provide some insights into how Watt was treated before the Edinburgh student audience through the early decades of the nineteenth century.

In a lecture on 'Vapourisation', the fundamentals of which were probably laid in the 1790s but which was continually modified and updated, Watt was identified as among those who had conducted experiments 'to discover the degree of elasticity or Expansive force, which the vapor of water possess when generated at different temperatures'.[67] The others names were Robison, Bettancourt, Dalton, Schmidt, Ure and Sothern [*sic*].[68] In the second part of his lecture material on vaporization Hope stated:

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[66] See the valuable discussion in Jan Golinski, *Science as Public Culture. Chemistry and Enlightenment in Britain, 1760–1820*, 1992, pp. 11–49. Golinski discusses these issues with reference to William Cullen and Joseph Black and finds them symptomatic of a gradual and halting working through of issues of public and private interest and the role of the professoriate in emergent academic chemistry.

[67] Thomas Charles Hope, Lecture Notes on Chemistry 1790–1842, Edinburgh University Library, Special Collections, Gen. 268–72 at Gen. 268, envelope 31.

[68] The name of Sothern [Southern] is added in pencil, probably later.

> Some Exper^{t} made by Dr Watt, demonstrate in a manner still more striking … that Vapor contains a vast store of [latent heat] which becomes apparent at the moment of condensation.
>
> Knowing well that water is converted into Steam at a Temp. much below its usual one, when freed from the pressure of the Atm[osphere] he was in hopes that he would save much fuel, if in those engines & operations, in which the production of Steam is concerned, he caused the formation of it to take place in vacuo.
>
> To submit this to the test of Expt he contrived an App[aratus] & distilled a quantity of water releived [*sic*] from the usual pressure.[69]

In a story familiar from the *Britannica* articles on heat and steam, it is recounted that Watt found that the production of steam at low pressure required the same input of heat the bulk of which reappeared as the latent heat of condensation. Watt, we are told, concluded from these experiments, against his initial expectations and hopes, that 'the same quantity of heat was required to form vapour in vacuo, as under the usual pressure, & that consequently the … same quantity of fuel would be requisite in both cases; & that there would be no economy in Distilling, or generating vapour in vacuo'.[70] This certainly presents 'Dr Watt' as an exemplary chemist and experimenter in developing the idea, submitting it to the test of experiment, accepting the verdict, and moving on. It was also, presumably, used as a potentially powerful example for the students of the relationship that could subsist between experimental inquiry and practical arts.[71]

Other of Hope's lecture notes pertain directly to the water question. In his 'Old Notes' on 'Hydrogene' and their successive revisions we find a simplification of the discovery story. In the 'Old Notes' the account is very similar to that given in Black's lectures. Of Watt it is noted that 'Mr Watt of Birmingham, at the time that Mr Cavendish was proving by Exp^{t} the fact, conjectured that water was actually the product.'[72] Watt's place in this account was small, and his 'conjectural' activity did not rate the same notice as the 'proving' experiments of Cavendish. In later versions of the notes, however, Watt was expunged entirely and the attribution becomes a rather bald statement:

> The discovery of this fact [the composition of water] is one of the most curious & important of modern chemistry. It is due to the late Mr Cavendish of Lond[on] a Gent^{n} of Noble family & great fortune & a profound Philosopher. This very interesting Discovery was made in the summer of the year 1781.[73]

The fact remains, however, that the documentary evidence that we have concerning the content of lectures on chemical topics in Scotland suggests that some modest

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69 Hope, Lecture Notes, envelope 31.

70 Thomas Charles Hope, Lecture Notes on Chemistry, *c*. 1790–1842, 'Vaporisation, 2^{d} Part', Edinburgh University Library, Special Collections, Gen. 268, envelope 32.

71 Robison had also emphasized this point in his edition of Black's lectures where he repeatedly referred to Watt as the 'pupil' of Black and attributed at least some of the success of Watt's steam-engine innovations to the science he derived collaboratively from Black.

72 'Hydrogene N 3^{d} Old Notes', Edinburgh University Library, Special Collections, Gen. 271, envelope 147.

73 'Hydrogene', Edinburgh University Library, Special Collections, Gen. 271, envelope 148.

early recognition of Watt's contribution to the discovery of the composition of water gave way, under curricular pressure if nothing else, to simpler, less historical expositions that gave the stage solely to Cavendish.[74] Even then, though, Watt's general reputation as a chemist, as a researcher on heat and steam, remained established in the oral university tradition, and through the encyclopaedic one. Many British chemical texts thrust Cavendish forward as the discoverer against the usurping French. Some ignored Watt in the process, but others gave him a joint, more often a subsidiary, role in the discovery. This provided some basis on which an attempt could be made to resurrect Watt's claims, though it was clearly to be an uphill struggle. We now turn to examine those who led that attempt, prominent among whom was a prodigal son.

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[74] Hope's lecture notes frequently included notes to himself on the need to condense and shorten material. He decided that the number of lectures devoted to heat was far too great given the rather different relation of that topic to the field of chemistry as it had evolved in the years after the adoption of the Lavoisian System. Eventually, heat left the chemical curriculum altogether in the lectures of William Gregory, Professor of Chemistry at the University of Edinburgh. See the text for the course, William Gregory, *Outlines of Chemistry*, 1845, vol. 1.

Chapter 5

Keeping Account: James Watt Jr and the Filial Project

> Depend upon it, My Dear Sir, that your appeal to my filial duties is not made in vain. It has been the study and endeavour of my life to cause justice to be done to my father's merits … [1]

Introduction

There can be little doubt that the 'water controversy' would not have been as significant an issue as it became in early Victorian Britain had it not been for the involvement of James Watt's eldest, and only surviving, son, James Watt Jr. Watt Jr devoted much time to propagating and defending his father's reputation. Apart from being extremely zealous in monitoring, criticizing and shaping writing about his father, Watt Jr also concerned himself with other commemorations, such as various monuments, their inscriptions, and, especially, the Greenock Public Library. Besides written and monumental memorials to the great engineer, the son seems to have regarded the business of Boulton, Watt & Co. as itself a kind of sacred trust. His concern for the economic success of that business and for its reputation was another way in which the son sought to keep the memory of the father alive.

Within the filial project so defined, the 'water controversy' was very important to Watt Jr because he took it as illustrating his father's 'philosophical character', a crucial ingredient of his standing as a public benefactor. Watt Jr's own writing on the water question was not extensive but he was constantly behind the scenes, consulting on, and contributing to, the work of Arago, Brougham and Muirhead. Like those writers, Watt Jr subscribed, I suggest, to an empirical view of the nature of discovery. Watt Jr's attitude in business and in 'literary' controversy was that 'the facts' were the key, and that he had them. He considered his father's claim to priority to be a clear-cut, empirically demonstrable one. The efforts of the supporters of Cavendish to show otherwise were, in his opinion, 'sophistical'.

First let us sketch Watt Jr's early years. From consideration of his family environment, education, early radicalism and prodigal return to his father's business, we can discern many of the roots of the mature man's attitude towards his illustrious father. We can thereby understand his actions in defending and promoting Watt's reputation in general, and his claim to priority in the water controversy in particular.

[1] James Watt Jr to François Arago, 29 September 1834, Watt Papers (Doldowlod), W/10.

Watt Jr's Early Years

Watt Jr was born in 1769, the year in which his father obtained the first patent on the condensing steam engine. His youngest years saw the death of his mother (née Margaret Miller) in 1773, the family move to Birmingham in the following year, and the inauguration of his father's partnership with Matthew Boulton. The loss of his mother and a strained relationship with his stepmother (Watt married his second wife, Ann Macgregor, in 1776) may have led to a disproportionate emotional investment in his relationship with his father.[2] The latter's severity towards him might also have contributed to Watt Jr's rebellion and break with his father in 1790 over the son's radical support of the French Revolution.

Eric Robinson[3] and, more recently, Peter Jones[4] have explored the education of the sons of Boulton and Watt, the sons who in their turn were to become business partners. Robinson showed the practicality of the education desired by Boulton and Watt for their sons. There was strong emphasis on modern languages, science and commerce. Watt Jr was first intended as an engineer and was sent by his father to John Wilkinson's Bersham iron works to acquire the skills of machine drawing and carpentry, as well as geometry, arithmetic, algebra and merchant's accounts. The classics could be studied but only in the little time thereafter available. Watt was less indulgent than Boulton of purely literary studies and dourly warned his son off light reading. Robinson describes Watt as a rather 'sour' father.

At the age of fifteen, Watt Jr was sent to Geneva under the eye of Watt's friend J.A. De Luc to attend lectures at the Academy there. His father continued to hector him by letter about rising early for study, economy in expenditure, and seriousness in reading matter. There was a small element of light relief when he was allowed to take fencing lessons in company with Joseph Priestley Jr, who was also in Geneva at the time. In late 1785 young Jimmy moved on his father's instructions to study with the Reverend M. Reinhard at Stadtfeld near Eisenach in Upper Saxony, there to learn German, and thence in 1787 to Freiberg's School of Mines. His return to England in late October 1787 met his father's stern demands that he account for his 'reckless' expenditure while overseas. His father also tackled him about accusations of insubordination to his stepmother. Watt determined that domestic peace and his son's future were best served by apprenticing him in the counting house of Taylor & Maxwell, Manchester fustian makers.

Watt Jr found Manchester an exciting place where he could develop his scientific activities and indulge his radical political enthusiasms. He came under the influence

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2 Years later, when Watt Jr was in his mid-60s, he sent to Arago a long extract he had found in his father's journal. This very moving passage, written when Watt was surveying for the Caledonian Canal in 1773, related to the circumstances of his wife's death. Watt Jr sent this to Arago to demonstrate 'the affectionate character of his [Watt's] mind'. It appears that even at this juncture it was important to Watt Jr to demonstrate the depth of his father's sense of bereavement. See James Watt Jr to François Arago, 22 January 1835, Watt Papers (Doldowlod), W/10.

3 Eric Robinson, 'Training captains of industry: The education of Matthew Robinson Boulton (1770–1842) and the younger James Watt (1769–1848)', *Annals of Science*, **10**, 1954, 301–13.

4 Peter M. Jones, 'Living the Enlightenment and the French Revolution: James Watt, Matthew Boulton, and their sons', *The Historical Journal*, **42**, 1999, 157–82.

of Thomas Cooper and Thomas Walker, both significant figures in Manchester business and in radical politics, through both the Manchester Literary and Philosophical Society, of which he became Secretary,[5] and the Manchester Constitutional Society, of which he was a founding member in June 1789.

The political rift between father and son widened. Though in many intellectual respects Watt shared the liberal Enlightenment values of his Lunar Society associates, he was more conservative than they.[6] While condemning the 'Church and King' mobs which sacked Priestley's house in Birmingham, he lamented the way that the times were making the Lunar Society meetings more affairs of politics than of philosophy. His son, and young friends, by contrast resigned *en masse* from the Manchester Literary and Philosophical Society when it refused to take a political stance by deciding not to send a message of sympathy to Priestley on the occasion of the Priestley Riots, as the Derby Philosophical Society had done.[7]

When Watt Jr's apprenticeship with Taylor & Maxwell ended he took a position with T. & R. Walker, textile merchandisers, as a commercial representative. The senior partner in this firm, Thomas Walker, was his political associate as well as his employer, and the two certainly saw travel on behalf of the firm and support of liberty as appropriately conjoined concerns.

In March 1792 Watt Jr, Thomas Cooper and John Tuffen landed at Calais and travelled to Paris. Although Watt Jr was representing his firm on business he was also intent upon supporting the cause of liberty in France. In Paris he renewed contacts including many of the leading chemists (de Morveau, Fourcroy, Lavoisier) though the latter were preoccupied with politics. Watt Jr and Cooper presented an address to the Jacobin Club of Paris from the Manchester Constitutional Society. Such fraternal acts of solidarity were commonplace. However, they were noted, and became the subject of a denunciation by Edmund Burke in the House of Commons on 30 April 1792. As Cobbett's *Parliamentary History* reported, Burke stated that:

> There were in this country men who scrupled not to enter into an alliance with a set in France of the worst traitors and regicides that had ever been heard of – the club of the Jacobins. Agents had been sent from this country, to enter into a federation with that iniquitous club, and those agents were men of some consideration in this country; the names he alluded to were Thomas Cooper and James Watt. Here Mr Burke read the

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[5] At this time Watt Jr was the author of two papers: 'Some account of a mine in which Aërated Barytes is found', *Memoirs of the Literary and Philosophical Society of Manchester*, **3**, 1789, 598–609 and 'On the effects produced by different combinations of the Terra Ponderosa given to Animals', idem, **3**, 1789, 609–18.

[6] Boulton and Watt had to moderate their radical political associations for commercial reasons since, whatever Watt might protest to the contrary, their business and its support (not least through patents) depended upon the political establishment. Only occasionally, when it seemed that that Establishment might fail him through its corrupt character, did Watt sound off against it, and then only in private to his wife. Thus, when it appeared that the House of Commons might pass Hornblower's bill, Watt wrote to Ann Watt: 'A little more of this will make me an enemy of corrpt p^{ts} [practices] and a democrate if democracy were less evil' (Watt to A. Watt, n.d. [March 1792], as quoted in Jones, 'Living the Enlightenment', p. 170).

[7] Eric Robinson, 'An English Jacobin: James Watt, Junior, 1769–1848', *Cambridge Historical Journal*, **11**, 1954–55, 351.

> address presented to the club of the Jacobins by those gentlemen on the 16th of April … And what did those people do? did they only give their own sentiments? No. By the answer of the Jacobin club, it appeared that those worthies of Manchester undertook– from what authority he knew not– to represent all England.[8]

Through the year 1792, reports of Watt Jr's activities in France caused consternation in the Watt household. Watt was anxious that the failure of reports to distinguish between himself and his son was harming his own reputation and that at a crucial time when parliamentary support for Watt against the Hornblowers on patent questions was vital.[9]

As the Revolution progressed, Watt Jr took a crash course in party division. He gravitated to the Brissotin wing of the Jacobins with their opposition to extra-parliamentary radicalism. Much to Watt's relief, business took his son out of Paris in September 1792 just as the events of the Terror escalated. Watt Jr proceeded via Nantes, Bordeaux, Marseilles, and Leghorn to Naples. However, their correspondence reveals a widening gulf of mutual political incomprehension. His son's continued allegiance to extreme democratic views (as he saw them) brought Watt to the verge of despair and abandonment, even as it became apparent that counter-revolutionary measures in Britain could well make the return of Watt Jr difficult, if not impossible.

Watt Jr considered joining the French army, but even his radical friends advised against that. The imprisonment and execution of many of the Brissotins was a cruel lesson in the realities of the Terror. Then, in October 1793, in the midst of his son's growing crisis of doubt, Watt suddenly softened his approach, advising his son not to give way to a 'fruitless despondence'. He encouraged him to stay abroad and provided him with money. Watt Jr accepted these overtures of goodwill gladly and the tide in their relationship seems to have turned in October and November 1793. Watt Jr appears to have realized his dire situation and welcomed his father's willingness to rescue him despite all their prior disagreements. His half-sister Jessie was dying from consumption at this time and this may have brought the family closer.

The reasons for Watt's change of heart are a matter for speculation. Jones suggests that he may have been alarmed by Watt Jr's melancholia or that he wanted to prevent his son joining radical members of their circle emigrating to the United States. Another possibility, Jones suggests, is that the partners Boulton and Watt were considering retirement and were anxious to settle 'the business on the next generation'.[10] As it turned out, Watt Jr returned to England very quietly early in February 1794, staying at first in London. Then in March he returned to Birmingham, where he lodged with Matthew Boulton. The relationship with his stepmother remained difficult and he agreed with his father not to seek to live under the same roof.

A few months later a new engine-building company was established in which Watt, Watt Jr, Gregory Watt, Matthew Boulton and Matthew Robinson Boulton

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[8] William Cobbett, *The Parliamentary History of England from the Earliest Period to the Year 1803*, vol. 29, 1817, pp. 1317–24, at pp. 1322–3. See also D.V. Erdman, *Commerce des lumières: John Oswald and the British in Paris, 1790–1793*, 1986, pp. 150–55.

[9] Jones, 'Living the Enlightenment', p. 172.

[10] Ibid., p. 179.

5.1 James Watt Jr

were all partners. Thereafter Watt Jr expended his energies on the family business. It is plausible that Watt Jr's devotion to that business and to his father's reputation drew extra strength from a relationship forged in extreme conflict, subsequent reconciliation and the realization on the son's part of the depth of his often distant and severe father's love for him.

Watt Jr, the Family Business and Intellectual Outlook

The first major assignment undertaken by Robinson Boulton and Watt Jr was to deal with engine pirates, especially in Lancashire. As Musson and Robinson put it, they were the 'generals in charge of the battle against the Manchester pirates' and the young Turks 'organised a complete system of espionage' in detecting pirate engines.[11] Watt, evidently delighted with their efforts, remarked that 'But for the good sense and indefatigable activity of Mr Boulton Junr and my son, we must have succumbed, for want of animal life and Spirits'.[12] Watt was clearly relieved that his errant son's energy and enthusiasm were now directed toward the support and advancement of the family enterprise.

Watt Jr was also intimately involved in the establishment of the Soho Foundry, which opened in January 1796. Boulton and Watt had relied heavily on John Wilkinson for the supply of components for their engines. Wilkinson had turned against them through acts of piracy of his own and there had been problems of supply and quality control with engine parts. These factors, together with the impending expiry of the engine patent in 1800, induced the company to engage more actively in engine manufacture than had previously been the case.

Watt was not a partner in the Soho Foundry, though Watt Jr and Gregory Watt were. In 1800, Watt retired from business entirely. With the death of Gregory Watt in 1804 and of Matthew Boulton in 1809, James Watt Jr and Matthew Robinson Boulton were left as the sole owners of the Soho Engine Manufactory and the Soho Foundry. The nature of the business was changing after the expiry of the Watt patent. The grounds of competition with other engine makers and suppliers changed. Boulton, Watt & Co., Soho undertook a range of initiatives in manufacture, organization and accounting, sales and service and marketing. Although some of these activities engaged Robinson Boulton, it was becoming apparent that Watt Jr was more committed to the business than his partner. Watt Jr's business and accounting skills were applied to an elaborate costing exercise in both the Engine Manufactory and the Foundry in the early nineteenth century. Watt Jr also provided much of the after-sales service to purchasers of the firm's engines.[13] Jennifer Tann observes that from that time, however, Watt Jr 'took the major management decisions in the firm'.[14] In later years Watt Jr was to speak of

[11] A.E. Musson and Eric Robinson, *Science and Technology in the Industrial Revolution*, 1969, pp. 413–14.

[12] James Watt to Joseph Black, 1 June 1796, quoted in Eric Robinson and Douglas McKie, *Partners in Science. Letters of James Watt and Joseph Black*, 1970, pp. 224–25.

[13] Jennifer Tann (ed.), *The Selected Papers of Boulton and Watt, Volume 1*, 1981, pp. 233, 276.

[14] Ibid., p. 30.

Robinson Boulton as a sleeping partner. Major initiatives such as a significant move into the manufacture of marine engines lay with Watt Jr.[15] All this encouraged Watt Jr to think of himself as the sole saviour of the company's name and the chief custodian of its reputation.

In his later years Watt Jr reflected on this directly. By the mid-1830s Robinson Boulton, who had long been relatively inactive in the business, decided to retire. Watt Jr resolved to 'take the whole upon myself'. As he explained to François Arago:

> I did hope that a Firm which has now lasted for more than 60 years, might have continued to the end of our respective lives, and I have made great sacrifices of time to the business, which is in a most prosperous state, to obtain that object. But … I have given way to his wishes. At my period of life, and with a fortune ample to my wants; it may seem to you an Act of folly, that I should continue to expose myself to the exertions which the management of such a business as ours requires, and I might add to its possible vicissitudes; but so it is, that I have an insuperable aversion to desert a concern, founded by my father, and which has contributed not a little to his reputation, and has placed myself in a forward rank among the Manufacturers of this Country. It has been the pride & pleasure of my life to continue & keep up that business … [16]

It was not until 1840 that Watt Jr became the sole owner of the foundry business on the dissolution of the partnership with Boulton. He quickly involved new partners: Henry Wollaston Blake (a London businessman and banker); and two former employees, James Brown and Gilbert Hamilton.[17] He was pleased with this arrangement, entered into in 1841. The unfortunate nature of his relationship with Robinson Boulton becomes apparent from Watt Jr's comments to Henry Brougham on his former partner's death:

> My early friend and partner for so many years, Mr Boulton, paid the debt of nature on the 18th Inst. … we have had no intercourse since the dissolution of our partnership, but I have remained upon friendly terms with his family. My resentments will now be buried with him, and I set out tomorrow for Aston hall to render what consolations I can to his family, and to attend his funeral.[18]

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[15] Ibid., pp. 17–18. Watt Jr pursued experiments on marine engines using the *Caledonia*, a ship purchased specially for the purpose from the steamship pioneer Henry Bell of Glasgow. In October 1817 Watt Jr sailed in the *Caledonia* to Holland and then up the Rhine as far as Coblenz, visiting Antwerp and Rotterdam before returning in the spring of 1818. Then he conducted an extensive series of experiments with the ship on the Thames that informed improvements in the construction and adaptation of marine engines. When he had finished with it, the ship was sold to the Danish government. See Martin Bellamy, 'P.S. *Caledonia*: Denmark's first steamship', *The Mariner's Mirror*, **80**, 1994, 55–58 and James Patrick Muirhead, *The Life of James Watt with Selections from his Correspondence*, 1858, pp. 442–44. See also Watt Jr to John Rennie, 3, 4, 6, 14, 15, 16 October 1817, National Library of Scotland, Rennie Manuscripts, MSS19824, ff. 116–25.

[16] James Watt Jr to François Arago, 8 November 1835, Watt Papers (Doldowlod), W/10.

[17] See W.K.V. Gale, 'Soho Foundry: Some facts and fallacies', *Transactions of the Newcomen Society*, **34**, 1961–62, 83.

[18] James Watt Jr to Henry, Lord Brougham, 21 May 1842, Brougham Papers, 20,148.

As he pondered the past at Robinson Boulton's graveside, Watt Jr probably considered himself as the last man standing among the company of those who had made the name of Watt famous in the land.

Let us now discuss what the educational and early formative experiences of Watt Jr meant for his stance in relation to the water controversy. Perhaps the first point to note is that the education that Watt Jr and the younger Boulton received was one of ardent rationalism and intense practicality. The link between philosophical, manufacturing and commercial concerns was made evident in Matthew Boulton's advice to his son:

> A man will never make a good Chymist unless he acquires dexterity, & neatness in making exp^ts^, even down to the pulverising in a Mortar, or blowing the Bellows, distinctness, order, regularity, neatness, & Cleanliness are necessary in the Laboratory, Manufactory, & the Counting house.[19]

These were sound proceedings, as a chemist drew upon the same virtues as informed successful manufacturing and commercial activity. Note also that esoteric knowledge was not particularly stressed, the implication being that someone possessed of the requisite practical skills and virtues gained in successful manufacturing or commerce would be able to deal in chemical matters. We have already observed (in Chapter 3) that the chemical traditions deriving from Joseph Black were fashioned by their adherents in the 1790s and 1800s as very much against the pursuit of systems of chemistry and for a sober experimental approach. This sort of notion informed Watt Jr's picture of his father as a chemist.

Peter Jones makes another interesting observation about Watt Jr and his generation. Their experiences helped to mark out their attitudes from those of their liberal fathers. In particular, there was in the generations after 1792 a weakening of ideals of free trade in knowledge. One sign of this was the decision by Robinson Boulton and Watt Jr to no longer allow visitors to tour the Soho Manufactory. Proprietary interests now decisively outweighed openness of communication: 'A new generation was in charge; one which no longer shared the reflexes of those who came to maturity in the high decades of the Enlightenment.'[20]

In 1841, Watt Jr's coadjutor in the filial project, James Patrick Muirhead, paid a visit to Watt Jr's old friend, the poet William Wordsworth, who recalled their young, radical days in France. As Muirhead reported the conversation with Wordsworth to his mother: 'They [Watt Jr and Wordsworth] thus both began life as ardent (and he adds, thoughtless) radicals, but have both become, in the course of their lives, as all sensible men he thinks have done, good sober-minded *conservatives*.'[21] Wordsworth greatly approved of Sir Robert Peel's administration. Watt Jr certainly shared this view.[22]

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[19] Matthew Boulton to Matthew Robinson Boulton, 19 December 1787, as quoted in Eric Robinson, 'Training captains of industry', p. 309.

[20] Peter M. Jones, 'Living the Enlightenment', p. 181.

[21] J.P. Muirhead to Mrs Muirhead, 1 September 1841, reproduced in Anon, 'A day with Wordsworth', *Blackwood's Magazine*, **221**, June 1927, 733.

[22] See James Watt Jr to Henry, Lord Brougham, 30 March 1842, Brougham Papers, 20,145.

Watt Jr's tough proprietorial approach showed in the way that he ran the business, in his spirited defence of his Radnorshire estates[23] from the encroachments of poachers, and, ironically, railway companies, and, not least, in the way he defended his father's claims to inventive and scientific glory. It showed also in Watt Jr's attitude to various institutional developments. He saw little value, for example, in Mechanics' Institutes. When Brougham tried to interest him in promoting an Institute in Birmingham, Watt Jr declared that he could not help, 'not being a convert to the advantages of the Institutions you recommend'. Watt Jr was happy with the system of training pursued for many years at Boulton, Watt & Co., which had been 'productive at all times of a number of able mechanics, who are now to be found in most parts of the Kingdom'.[24]

Watt Jr and his loyal Soho engineers often exhibited a strong antipathy towards the organized engineering profession. This was linked in Watt Jr's mind to the activities of the 'pirates' who had plagued his father. Stimulated by a strongly negative review of the translation of Arago's *Eloge* in the 'Engineer's Journal' in 1839, Watt Jr recalled that 'there has at all times, within my Memory, been a spirit of jealousy among this class against the whole race of the Soho Engineers, who in return have treated them with due contempt … '.[25] Watt Jr then provided a very interesting insight into a publication in the *Edinburgh Review* in 1809 ostensibly by John Playfair. He recalled that the engineering opposition

> stirred up Olinthus Gregory a Professor at Woolwich to stand forward as their champion; to claim for them sundry inventions originating with my father or ourselves, & to vituperate others. That <u>roused</u> my choleric and with the assistance of Professor Playfair and the connivance of Jeffrey, I put forth the <u>Olynthiad</u> in the Edinburgh Review … That proved a settler for some years, and although they revived afterwards, we never troubled ourselves more about them. They are now dazzled & confounded by the excess of praise which Arago has bestowed on my father, and though they cannot contest the truths he brings forward, they employ some pettifogging hireling scribbler to undermine them by sarcasm & insinuation.[26]

Watt Jr doubted the disavowals of any knowledge of the review by James Walker, then President of the Institution of Civil Engineers, or as Watt Jr referred to him,

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[23] Watt had bought a significant landholding in Wales between the settlements of Rhayader and Builth Wells that included Doldowlod Farm. Watt Jr prided himself upon continuing and advancing the work that his father had begun in improving these estates. He was locked for some years in legal battles with encroachers upon these lands and was also upset by the prospect of railway companies acquiring a right of way through them.

[24] James Watt Jr to Henry Brougham, 17 October 1824, Brougham Papers, 27,390. On training at Soho see also Sidney Pollard, *The Genesis of Modern Management*, 1965, pp. 175–76.

[25] Watt Jr to Muirhead, 3 November 1839, Muirhead Papers, MS GEN 1354/405. The review to which Watt Jr referred was in *The Civil Engineer and Architect's Journal*, **2**, 1839, 399–419. It gave extensive translated extracts from Arago's *Eloge* and substantial commentary of a rather sarcastic nature by the translator. It was very critical of Arago and dismissive of the claims for Watt regarding the composition of water.

[26] Watt Jr to Muirhead, 3 November 1839. The article referred to as 'the Olynthiad' was published as: [John Playfair], 'Account of the steam engine', *Edinburgh Review*, **13**, January 1809, 311–33. Gregory responded in the *Monthly Magazine* (August 1809) and Playfair et al. retorted in *Edinburgh Review*, **15**, October 1809, 245–54.

'Chief of the Mongrel Engineers'. One detects in Watt Jr's perception of the relationship between the Soho Engineers and the organized profession the usual whiff of paranoia. This episode certainly provides further evidence for Hugh Torrens's depiction of Watt Jr and Watt himself as ruthlessly manipulating publications in the cause of Watt's priority.[27]

Watt Jr also strongly disapproved of the way that scientific bodies such as the British Association cavalierly communicated what he regarded as commercially confidential information.[28] It was as if the experience of fighting engine pirates in the immediate aftermath of his reconciliation with his father led him to see pirates everywhere subsequently. This, I believe, gives us some insight into Watt Jr's hypersensitivity where matters of paternal reputation were concerned and, within that, his long-standing preoccupation with credit in the 'water controversy'. If I am right that the psychological springs in these varied contexts were the same, then it is perhaps no surprise that the conceptual structure and method of argument (that of the accountant and legal prosecutor) was also the same in those different contexts.

The Filial Project

In August 1819, James Watt lay dying and the vicissitudes of his final illness were lovingly recorded in correspondence between his son and John Rennie.[29] On the day after his father's death Watt Jr wrote:

> I have indeed lost a father whom I deeply reverenced and dearly loved, and whose affection I enjoyed in return. Long years of intimate communication had sunk the father in the friend, and the loss of such a father and such a friend cannot but leave a void that I must strongly feel … It remains for me now to pay due honours to his memory … [30]

There was a great variety of occasions on which Watt Jr actively engaged in the cultivation of his father's reputation or assisted the efforts of others. By the 1840s some of Cavendish's supporters detected the influence of Aston Hall everywhere,

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[27] See Hugh Torrens, 'Jonathan Hornblower (1753–1815) and the steam engine: A historiographic analysis', in Denis Smith (ed.), *Perceptions of Great Engineers: Fact and Fantasy*, 1994, pp. 26–27.

[28] Watt Jr was not impressed by the exhibition of manufactures at the Birmingham meeting of the BAAS in 1839, that same meeting at which Harcourt assailed his father's scientific reputation. He maintained that such exhibitions were of no use in originating inventions: 'the sooner we resume our ancient habits of privacy and exclusion, particularly with regard to such processes as we may still alone possess, the more it will tend to the advantage of our manufacturing interests. And narrow minded as these opinions my appear to the members of the British Association and to some of our political economists, we hope that our warning voice may not be raised in vain, to prevent a repetition of such suicidal folly.' See Watt Jr, 'Note upon the Meeting of the British Scientific Association at Birmingham', n.d. [early September 1839], as quoted in Jack Morrell and Arnold Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science*, 1981, p. 264.

[29] National Library of Scotland, Rennie MSS 19824, ff. 132–33, 134–35, 138–39.

[30] Watt Jr to John Rennie, 26 August 1819, National Library of Scotland, Rennie MSS 19824, ff. 153–54. Rather poignantly, this letter was signed for the first time, 'James Watt' rather than 'James Watt Jr'.

monitoring, parrying and seeking to stifle criticism of the 'great steamer'. Watt Jr certainly spent over forty years of his life engaged on the filial project in one form or another. Before his father's death Watt Jr assisted with some of the autobiographical work in which Watt sought to enforce his version of events regarding his major inventions and his relations with Joseph Black. Various incidents in the immediate aftermath of his father's death indicate that the strong urge remained to control what was said about him, particularly so far as priority was concerned but also in regard to Watt's early life and background.

In the *Monthly Magazine* for 1 October 1819 a memoir of James Watt appeared by Mr William Playfair. A substantially different, but identifiably related, memoir appeared in the *New Monthly Magazine* two months later.[31] The proprietors of that last august journal advised readers that, although the memoir appeared in its rival journal first, it had been contracted originally for itself, with Playfair receiving 'a handsome remuneration'. It was further explained that while the memoir was 'in the hands of a friend of the late Mr Watt, for revision, the writer thought proper to dispose of a copy of the same to the old Monthly Magazine'. The editor left the public to 'form their own opinion on the conduct of Mr. Playfair'.[32]

The 'friend' to whom the editor submitted the article was James Watt Jr, who strongly disapproved of it. The version that finally appeared in the *New Monthly Magazine* was altered substantially. Nevertheless, Watt Jr was livid. Playfair himself sent a copy of the memoir to Heathfield *after* it had been published in the *Monthly Magazine*. Even worse, it appears that Playfair concocted a letter to himself from a 'John Smith' offering to withhold from publication certain 'facts' about Watt for a consideration. How the attempted blackmail ended up is unclear.[33] Watt Jr was certainly persuaded of the need to make sure that press accounts of his father were, wherever possible, scrutinized before publication. In an exchange with the publishers of the *Annual Obituary*, whose obituarist had relied on Playfair's memoir, Watt Jr rejected the result entirely and sent a composition of his own.[34]

Watt Jr began to familiarize himself with the papers that his father had left. One of the surprises he found was a bundle of correspondence concerning the water

31 William Playfair, 'The late JAMES WATT, Esq, F.R.S. &c &c', *Monthly Magazine*, 1 October 1819, 230–39; idem, 'Memoir of James Watt Esq. F.R.S.', *New Monthly Magazine*, **12**, December 1819, 576–84. Playfair was a brother (evidently a black sheep) of Professor John Playfair, a long-standing friend of the Watt family.

32 *New Monthly Magazine*, **12**, December 1819, 576.

33 See William Playfair to Watt Jr, 12 October 1819, 22 October 1819; H. Colbourn to Watt Jr, 11 September 1819; John Rennie to Watt Jr, 11 September 1819, 25 October 1819, 1 November 1819; Watt Jr to John Rennie, 14 October 1819, all in Watt Papers (Doldowlod), C6/10. This also contains the letter from 'John Smith', dated London, 7 October 1819. The letter retails claims about the manner in which Boulton accumulated and held onto his fortune at the expense of others and accuses Watt of complicity in this. It also remarks on Watt's meanness and claims that 'Murdoch not Watt improved the Engine. As to the Crank it was no Invention at all and the Great Improvement in Execution Mr Watt had nothing to do with.' Other material on the affair includes: Watt Jr to John Rennie, 10 September 1819, 21 October 1819, 28 October 1819, National Library of Scotland, Rennie Papers, 19824, ff. 155–56, 160–61 and 164–65.

34 See Messrs Longman & Co. to Watt Jr, 21 December 1819 and 4 January 1820; Watt Jr to Messrs Longman & Co., 1 January 1819 [1820]; Watt Papers (Doldowlod), C6/10.

controversy. The normal process of collecting obituaries of his father expanded into critiques of them and a resolution that a proper biography should be undertaken. 'It is my intention', Watt Jr wrote to John Rennie, 'to devote all my spare time to the collecting and arranging materials for a history of my father, as soon as I can set about it.' An early move to engage John Barrow of the Admiralty to write the biography came to nothing.[35]

In 1823 Watt Jr agreed to write an account of his father for the *Supplement* to the fourth, fifth and sixth editions of the *Encyclopaedia Britannica* then being produced under the editorship of Macvey Napier. The *Britannica* had included biographical articles from its second edition but Napier was also adding biographical articles on recent lives.[36] Napier approached Watt Jr in May 1823 to undertake the life of his father at a point when the *Supplement* was almost complete. The entry for 'James Watt' would appear in the final half-volume. The editor made it clear that the article would have to be of four or five pages at most, be confined to 'such particulars as are most useful and proper to be inserted in a work of reference', and would be due in October. Napier explained that he was approaching Watt Jr in order to obtain a definitive account: 'I am the more anxious to get the article from an authentic and authoritative quarter, from having been informed, that some inaccurate allusions to some part of your father's life, in an earlier portion of the work, had given pain … '.[37] Napier appeared to recognize that with the biography of Watt he was on sensitive territory, being monitored by Watt Jr, so why not approach the possessor of the filial 'eagle eye' directly?

Napier waited six weeks for a diffident reply from Watt Jr, who explained that he had been working on a memoir of his father as a larger project but was constantly diverted by business. Uncertain whether he could supply the article in time, he would do his best to provide a brief statement of Watt's inventions. As to previous errors: 'My father was convinced that no intentional error respecting him was introduced into your publication, and I have always considered myself and the public indebted to it for having called from my father himself that correction & narrative which is given in the republication of Professor Robison's Tracts.' On 1 September a nervous and uncertain Napier wrote to check that he and Watt Jr did in fact have an agreement about the article. He reiterated the shortage of space, the necessity of sticking to the key facts and he also suggested that Watt Jr might make some use of Francis Jeffrey's 'admirable character of your father'.[38] This was a reference to the obituary of Watt written by Jeffrey for *The Scotsman* newspaper. In late October, when the deadline had passed, Napier

[35] Watt Jr to John Rennie, 14 October 1819, Press Copy, Watt Papers (Doldowlod), C6/10.

[36] See Richard Yeo, 'Alphabetical lives: Scientific biography in historical dictionaries and encyclopaedias', in Michael Shortland and Richard Yeo (eds), *Telling Lives in Science: Essays on Scientific Biography*, 1996, pp. 155–61. See also Richard Yeo, *Encyclopaedic Visions: Scientific Dictionaries and Enlightenment Culture*, 2001, pp. 260–64.

[37] Macvey Napier to Watt Jr, 9 May 1823, Watt Papers (Doldowlod), C6/10. The inaccuracies referred to were those in the articles on 'Steam' and 'Steam Engine' by John Robison, which had been the subject of significant 'correction' by Watt himself in 1813–14. See Chapter 7 for more details.

[38] Watt Jr to Napier, 19 June 1823 and Napier to Watt Jr, 1 September 1823, Watt Papers (Doldowlod), C6/10.

had still received nothing. He could allow another few weeks but must have an assurance that Watt Jr would produce the article. That assurance was finally forthcoming.[39]

Watt Jr eventually sent the memoir of his father to Napier on 22 December 1823, explaining that he had been as brief as possible. He had tried to describe 'the history and nature of his principal Inventions, give little more than an enumeration of the rest, and a short notice of the leading events of his life'.[40] Two days later, on Christmas Eve, Watt Jr forwarded some additions and amendments and yet more on Boxing Day. Napier must have had mixed feelings when Watt Jr helpfully advised that he was having the memoir written out again with the amendments and additions included. Still further revisions followed on New Year's Eve.[41] Amidst this flurry and fuss of amendments Napier praised the article as exactly the sort of account that suited the work in which it was to appear.[42] Perhaps he hoped to stem the flow of additions and amendments from Aston Hall that threatened to continue unabated. These were the last thing Napier needed. He did agree, however, to try to edit other articles down in order to make room for the extended version. Against usual practice, Napier also granted Watt Jr copyright on the piece.

Two other issues were debated in these epistolary exchanges – the question of identifying Watt Jr as the author, and the content of another biographical article in the *Supplement*.[43] Watt Jr wanted to remain anonymous:

> I think it will not answer any good purpose to make it known I am the author. From the space to which I was limited the present memoir necessarily contains many assertions unaccompanied by the proofs, and until I am able to give both together in a longer work, I should wish to withhold my name; as what is said with the intention of doing justice to my father's merits, might be considered in me the result of partiality & prejudice, until it can be substantiated by the original documents.[44]

Napier tried to dissuade the bashful contributor. Named authors were very important to him commercially.[45] He asked why Watt Jr had such scruples: 'It is <u>impossible</u> that you can praise your father too much. Praise from you is not only natural, & graceful, but most proper. You cannot publish an <u>extended</u> memoir without speaking of him as you think right. What objection then, can there be, to do this, in the present short one?'[46] Napier indicated that if anonymity was insisted upon he must be able to say at least that the memoir came from a '<u>near relative</u> of Mr Watt' in order that the authoritative character of the article be

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39 Napier to Watt Jr, 26 October 1823 and Watt Jr to Napier, 30 October 1823 (Copy), Watt Papers (Doldowlod), C6/10.

40 Watt Jr to Napier, 22 December 1823 (Copy), Watt Papers (Doldowlod), C6/10.

41 Watt Jr to Napier, 24, 26 and 31 December 1823 (Copies), Watt Papers (Doldowlod), C6/10.

42 Napier to Watt Jr, 26 December 1823, Watt Papers (Doldowlod), C6/10.

43 Napier to Watt Jr, 3 January 1824, Watt Papers (Doldowlod), C6/10.

44 Watt Jr to Napier, 31 December 1823 (Copy), Watt Papers (Doldowlod), C6/10.

45 On the importance of 'names' in marketing the *Britannica* see Yeo, *Encyclopaedic Visions*, pp. 257–59.

46 Napier to Watt Jr, 3 January 1824, Watt Papers (Doldowlod), C6/10.

appreciated.[47] However, Watt Jr stood firm, asking that any allusion to the author be 'as little definite as possible', if it could not be passed *sub silentio*.[48] Watt Jr's reticence is perhaps best explained by his expectation of a hostile reception from certain quarters to his father's claims to invention and discovery and his anticipation, therefore, that his statements would be regarded, and dismissed, as interested and biased. In those circumstances he wanted to be identified only when he was free to make the best and fullest case, incorporating all available evidence. Such a case would, he hoped, be beyond reproach or challenge. It was to be many years before that case was mounted.

Watt Jr largely had it his way. He substantially exceeded the four or five pages stipulated. The additions were all included. His anonymity was preserved – just. A note at the end of the memoir stated: 'The Editor has received this article from a quarter which entitles him to state, with the utmost confidence, that it contains an accurate and faithful account of Mr Watt. The brilliant eulogium with which it so properly concludes is known to have been written by Mr Jeffrey.'[49]

The other matter negotiated by Napier and Watt Jr at this time confirms the overweening preoccupation with his father's reputation. It concerned an earlier biography in the *Supplement* of John Rennie (1761–1821) by John Barrow. Watt Jr had been presented with a copy of this biography by Rennie's son. Predictably, he took exception to a statement made there about the elder Rennie's position *vis-à-vis* the firm of Boulton & Watt. The article also contained a statement about Boulton and the Mint to which Robinson Boulton took exception. Watt Jr wanted a correction. In fact he wanted the article to be rewritten and he and Robinson Boulton had gathered materials to help Barrow to do that. Napier was aghast at this suggestion. He was quite happy as editor to include a note in the article on Watt correcting the mistakes and observed that the corrections would appear at the same time as the mistakes.[50] Watt Jr refused to back down. He volunteered to make good any financial cost that might be involved in producing a revised life. Robinson Boulton and Watt Jr approached Barrow about it, who took umbrage. He considered that they were 'meddling with things' that did not concern them, and thought Watt Jr 'the most obstinate & wrong-headed man I ever met with – different, very different, in all respects from his father … '. Barrow left it to the Rennies what to do. They decided to comply.[51] One begins to see why, over the years, the dogged insistence of Aston Hall, backed by determination and financial resources, came to be regarded as an almost pathological exhibition of filial piety.

In 1824, Watt Jr was also active in the shadows of the great meeting at the Freemasons' Tavern to open a subscription for a monument to his father. That

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47 Ibid.

48 Watt Jr to Napier, 10 January 1824 (Copy), Watt Papers (Doldowlod), C6/10.

49 See 'James Watt', *Supplement* to the fourth, fifth and sixth editions of *Encyclopaedia Britannica*, vol. 6, pp. 778–85, at p. 785.

50 Watt Jr to Napier, 22 and 31 December 1823; Napier to Watt Jr, 26 December 1823 and 3 January 1824, Watt Papers (Doldowlod), C6/10.

51 John Barrow to George Rennie, 27 November 1823, National Library of Scotland, MSS 19938, ff. 38–39 and ff. 40–41; Barrow to George Rennie, 7 January 1824, Macvey Napier Correspondence, British Library Add. MSS. 34,611, ff. 217–18.

meeting was chaired by Charles Hampden Turner, a family friend, and addressed by numerous luminaries.[52] The massive sculpture by Francis Chantrey that resulted from this process was finally installed in Westminster Abbey in 1834 and provided a fascinating episode in the filial project. Henry Brougham had agreed to write the inscription for this monument, an activity for which he was already quite renowned. He had written an inscription for a statue of Watt given by Watt Jr to Glasgow University.[53] Watt Jr requested a number of changes to Brougham's draft of the Westminster inscription.[54] He silently changed his father's birthdate and age since Brougham, who was not renowned, as we shall see, for attention to detail, had them wrong. Watt Jr wanted the line 'Educated at Glasgow' removed, as untrue:

> He [Watt] has himself stated in his letter to Dr Brewster, prefixed to the latters edition of Dr Robison's Essay on the Steam Engine, that he never attended any lectures at the College of Glasgow, & I have heard him say that the only tuition he received, was at the Grammar School of Greenock.[55]

The most interesting and telling change that Watt Jr requested concerned the line initially rendered by Brougham as 'Happily trained in philosophic research'. The words 'Happily trained', Watt Jr suggested, were inappropriate given the general understanding of training as 'instruction given or directed by others'.

> Upon reading the Memorials of Dr Black & Mr Robison, your Lordship will perceive that when my father first became acquainted with the Professors at Glasgow, in the year 1757, he being then 21, he was already qualified in point of scientific attainments to be their associate, and possessed a knowledge of the arts which they had not. I believe that independent of the tuition of the Grammar school of Greenock, which from ill health he was little able to attend, and a years practice with a Mathematical instrument maker in London, all his knowledge was acquired by his own reading and observations and reflections. It strikes me therefore that for the above two words you might substitute 'early devoted', 'early turned' or 'early applied'.[56]

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52 For the speeches made on this occasion see François Arago, *Historical Eloge of James Watt … Translated … by James Patrick Muirhead*, 1839, pp. 183–239.

53 See James Watt Jr to Henry Brougham, 31 March 1833, Brougham Papers, 44,876.

54 The version that Watt Jr corrected originally read thus: 'Not to perpetuate a name/Which must endure while the peaceful arts flourish/ But to testify/That mankind can distinguish those/Who have best earned their gratitude/The King/With many of the most eminent individuals in the Realm/Raised this monument to/James Watt/LLD FRS/Who by applying with unparalleled success/A great original Genius/Happily trained in Philosophic research/To the improvement of the Steam Engine/Enlarged the resources of his country/Increased the power of man/And exalted himself/To the highest place among the followers of science/And the real benfactors of the world/He was born at Greenock in MDCCXLI/ Educated at Glasgow/Died at Heathfield near Birmingham/MDCCCXIX/Aged/LXXVII'. See James Watt to Henry Brougham, 13 July 1834, and enclosure, Brougham Papers, 27,513.

55 James Watt to Henry Brougham, 13 July 1834, Brougham Papers, 27,513.

56 James Watt Jr to Henry Brougham, 14 March 1835, Brougham Papers, 27,743. Brougham agreed to the change though it is hard not to see in his original wording a concern to assimilate Watt to the Mechanics' Institute and SDUK movements with which Brougham was so closely involved. The final version of the inscription read: 'Not to perpetuate a name/Which must endure while the peaceful arts flourish/But to shew/That Mankind have learnt to honour those/Who best deserve their gratitude/The

One of Watt Jr's primary concerns, then, was to get a clear statement depicting his father as an autodidact. It was important that the great engineer's mechanical and philosophical genius be seen as original and native to him.

This was a point that Watt Jr insisted upon on all occasions. Many of Watt Jr's points of correction to Arago's *Eloge* were to concern this same question. The merest whiff of the idea that his father depended on others evoked Watt Jr's protests. For example, the hapless James David Forbes wrote a memoir of John Robison in which he mentioned that Robison had rendered great service to Watt in his lawsuits. On receiving a copy of this document, Watt Jr generally praised it but then rather curtly observed: 'I am not aware upon what grounds you state, in alluding to the mutual friendship which existed between Dr Robison and My Father, that "by far the greatest services" were conferred by the former. And you will oblige me by explaining to what you refer.'[57] Somewhat taken aback, Forbes wrote to Robison's widow: 'Mr Watt, I find, most unexpectedly … has taken umbrage at the expression used in my Memoir of Sir J. Robison … intimating that Mr Watt, senior, received great benefits at the hands of Dr Robison; by which I referred chiefly to his support of Mr Watt's Patent Right.'[58]

In responding to Watt Jr, Forbes offered a similar clarification:

> In saying (as I did advisedly) that 'by far the greatest services, in the ordinary acceptation of the terms, were conferred by Dr Robison on Mr Watt', I referred more particularly to Dr Robison's exertions and important evidence on occasion of the Patent Trial, as well as to the valuable articles in the Encyclopaedia Britannica in which Dr Robison preserved so admirable a record of Mr Watt's Discoveries. I have not attempted to decide which of these eminent friends derived most intellectual profit and enjoyment from each others Society,- but by '<u>services in the ordinary acceptation of the term</u>' I referred to the promotion of Fortune & Fame, in which respects Dr Robison was able to confer a

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King/His Ministers and many of the Nobles/And Commoners of the Realm/Raised this monument to/ JAMES WATT/Who directing the force of an original genius/Early exercised in philosophic research/ To the improvement of/The Steam Engine/Enlarged the resources of his country/Increased the power of man/And rose to an eminent place/Among the most illustrious followers of science/And the real benefactors of the World/Born at Greenock MDCCXXXVI/Died at Heathfield in Staffordshire MDCCCXIX'. This statue of Watt, and a photograph of the marble inscription stone, were to be seen in November 2002 at the Scottish National Portrait Gallery in Edinburgh. The statue is on loan to the Gallery for a time before being transferred to its new owner, Heriot-Watt University.

57 James Watt Jr to James David Forbes, 13 March 1846, Forbes Papers, Incoming Letters 1846, no. 22.

58 James D. Forbes to Mrs Robison, 16 March 1846, Forbes Papers, Letterbook IV, p. 93. Mrs Robison's reply, written in a very old, spidery hand, is of interest even though it somewhat misconstrues the nature of Watt Jr's concern: 'I am sorry you have had any Trouble with Mr Watt's supposed offence. I attend'd Mr Robison to London and all was pleasant in the Meeting. If Mr Watt, Junior, had been there Himself, he must have thought so. The scientifick Gentlemen were all interested to convince the least informed of the Jury, that all was compleat, and, perfect, in the patent of Mr Watt, and, even the Judge himself was much pleased, as He said, He was too little of an Artist, to form an opinion of his own. In this simple statement I cannot see any help required or given, except Mr Robison's exertion in complying with Mr Watts wish, He should attend the Tryal: Mr Rs bad health made it difficult (but happily had no bad effect). If you chuse to forward this paper, that I never knew any wish or attempt to detract from his worthy Fathers indisputed Talents & merits In which by personal knowledge has long been esteemed & admired by, R. Robison' (R. Robison to James David Forbes, [?] March 1846, Forbes Papers).

> substantial benefit on his friend, which the latter, to the close of his life, gratefully acknowledged.[59]

Watt Jr at this time was deep in a 'campaign', as he called it, defending his father, himself, Brougham and Arago from the strictures of an acerbic article in the *Quarterly Review* written anonymously by George Peacock, one of the more strident of Cavendish's defenders.[60] It seems likely that, being busy defending his father's originality on the water question, Watt Jr had assumed that Forbes (a known associate of the Cavendish camp) was making a parallel attack on the originality of his father's improvements to the steam engine. Robison had been a witness to those improvements and, it was widely known, discussed them with Watt at the time they were first conceived. So it appears that on this occasion, too, Watt Jr, by now close to the end of his life, almost blind, and in very poor health, was still resisting the slightest hint that his father's inventions and discoveries were the product of anything but his native, untutored and unaided genius.

The Water Controversy and the Filial Project

Watt Jr is the 'Mr Gradgrind' of the water controversy. The appeal to the 'facts of the case' came most insistently from him. He believed that establishing the facts would settle the priority issue in the same way that the accounting ledger would settle matters of business. The publication projects, including the article on his father for the *Supplement* to the *Encyclopaedia Britannica* and the publication of the *Correspondence*, were conceived as getting the facts out into the open. Information and advice to Arago during the production of the *Eloge* were cast as getting the facts straight. Watt Jr's anger at the defenders of Cavendish was directed at what he believed was their sophistry in arguing around, and in spite of, the facts. He adopted the same attitude as his father to the idea that posterity could decide on the basis of the facts. Just as his father did not need to be trained to see the facts of chemistry, so, Watt Jr contended, he – and indeed anyone else willing to see – could discern the facts of the priority dispute. In terms of the attributional strategies discussed in Chapter 2, Watt Jr definitely employed synonymity and priority. That is, he assumed that there was no significant difference between the claims made by his father and by Henry Cavendish. The only issue was who got there first. The merits of the ideas promulgated, or their place in a train of research, are of little relevance on this view. Who and when are more important than the details of what. There is a marked similarity between Watt Jr's ideas about how a priority dispute should be adjudged and how a business problem should be solved, or a series of experiments on steamship engines conducted. The rigorous pursuit of the facts by keeping account was the key.

Watt Jr's memoir in the *Supplement* to the *Encyclopaedia Britannica* was his first public statement on the water question. What does he have to say there? First,

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[59] James David Forbes to James Watt Jr, 17 March 1846, Forbes Papers, Incoming Letters 1846, no. 23.

[60] [George Peacock], 'Arago and Brougham on Black, Cavendish, Priestley and Watt', *Quarterly Review*, **77**, 1845, 105–39.

it should be noted that the article, even when not directly discussing the water controversy, was concerned to assert Watt's credentials as a philosopher. The memoir began with the words 'James Watt, a Philosopher, mechanician and Civil Engineer … ', sobriquets that were usually reversed in other accounts, if 'philosopher' was used at all.[61] Watt's experimental investigation of the consumption of steam in the model engine at Glasgow University was conducted, we are told, in a 'truly philosophical manner'. Referring to the investigations and inventions involved in developing his rotative engines, the memoir stated: 'we are impressed by a union of philosophical research, of physical skill, and of mechanical ingenuity, which has, we believe, no parallel in modern times'.[62] Thus were the philosophical credentials asserted.

The memoir devoted just over two pages to the water question itself. The claims made are as follows. First, that in early 1783 Watt reached the conclusion from Priestley's experiments that 'water is a compound of dephlogisticated and inflammable airs (as they were then called) deprived of their latent or elementary heat and he was the first to make known this theory', in a letter to Priestley dated 26 April 1783. Second, that the letter circulated in London among members of the Royal Society including Joseph Banks and Charles Blagden, but before the letter was read to the Society Watt asked for a delay in its reading because of 'new experiments' by Priestley. Third, that Watt sent a revised edition of his letter to De Luc on 26 November 1783 which was read to the Royal Society on 29 April 1784 and published in the *Philosophical Transactions* of that year. On 15 January 1784 Cavendish read a paper concerning his experiments which, it is claimed, drew 'the same inference as Mr Watt; with this difference only, that he did not admit elementary heat into his explanation'. It was also claimed that Cavendish knew of Watt's paper when his (C's) paper was read, stated that his experiments were made in 1781 but said nothing about when he formed his conclusions, and stated that a friend of his (Blagden) gave an account of his experiments and the conclusion drawn from them to Lavoisier in the summer of 1783. Finally, the memoir contended that Watt had not heard of Cavendish's experiments when he drew his conclusion.

This represents a brief, clear statement of the empirical position. The documented facts are treated as clear: that Watt first drew the conclusion; that the conclusion did not differ significantly from the conclusion later announced by Cavendish (and therefore was the *same* and not a different, or inferior, or the wrong conclusion). So far as Cavendish is concerned, the facts presented are used to imply that he did not have a conclusion when Watt did, may have arrived at it later and separately, or may have derived it from Watt. In any case, Cavendish did not make any conclusions public before Watt. Even as this story was elaborated during the course of the controversy, the central features of it remained in all the Watt camp's effusions.

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[61] The British Biographical Archive provides a handy collection of biographies of Watt that can be compared with each other in this regard. See also the discussion of encyclopaedia entries in Chapter 11, and my article '"Puffing Jamie": The commercial and ideological importance of being a "philosopher" in the case of the reputation of James Watt (1736–1819)', *History of Science*, **38**, 2000, 1–24.

[62] See [James Watt Jr], 'Memoir of James Watt', *Supplement* to the fourth, fifth and sixth edition of the *Encyclopaedia Britannica*, vol. 6, p. 781.

A note at the end of the section dealing with the water question referred to the confusion of dates of Watt's letter to De Luc (regarding when it was written and when it was read to the Royal Society), expressing calmly an inability to explain this. It also stated: 'It is also a circumstance not to be passed over, that Mr Cavendish circulated the copies of his paper given him for private distribution with the erroneous date on the fly leaf "read 15th January 1783"!'.[63] These issues were to receive considerable elaboration, especially by Lord Brougham, but the implication of bad faith on Cavendish's part was already there.

Twenty years passed before Watt Jr appeared in print again on the water question. By 1846 when the *Correspondence of the late James Watt on his discovery of the theory of the composition of water* was published, Watt Jr was in poor health and almost blind. Muirhead did the major work of editing and introducing the *Correspondence*. Nevertheless Watt Jr exercised close supervision. He contributed a prefatory letter and this provides our second opportunity to discern Watt Jr's public stance on the water question. In consigning to Muirhead the task of editing and introducing the *Correspondence*, Watt Jr presented the exercise as an evidential one that 'falls peculiarly within the sphere of your pursuits'.[64] Chemical knowledge might be useful, and Watt Jr recalled that he himself was 'tolerably versed in the facts and doctrines of the new system of chemistry, which the able writings and generalization of Lavoisier had caused to be commonly received'.[65] However, the skill of lawyers such as Muirhead (and Brougham) in considering and weighing evidence was, Watt Jr considered, the most important qualification.

Having recounted his consultations about the correspondence with various parties over the years, to which we will turn in a moment, Watt Jr then recalled his memoir in the *Britannica*. He reproduced the text of that section of the memoir which dealt with the water question. He described this text as 'necessarily somewhat imperfect' because 'the whole of the facts since ascertained' were not known to him then.[66] What facts did he mean? He did not notice any correction or addition to the basic elements of the empirical account that we outlined above. What he *did* do, however, was discuss the additional information that Lord Brougham had uncovered about the Cavendish camp's supposed fiddling with documents. He also reported the negative outcome of investigations in the Cavendish papers seeking to find an earlier statement of *conclusions* by Cavendish. The overall message, then, was that the facts regarding priority had long been established, that they were rooted in the public documentary base but decisively confirmed by the private correspondence now published. Watt Jr's own memoir, Arago's *Eloge* and Brougham's account in *Lives of Men of Letters and Science* were all grounded in that documentary base. Harcourt's pronouncements were made in ignorance of it, Watt Jr claimed.

The long hiatus between Watt Jr's literary interventions was not a period of inattention to the water controversy on his part. He recounted in his Preface the original discovery, in August 1819, of the water correspondence bundled among his

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63 It appears from the copy of the memoir in the Watt Papers (Doldowlod), 3/26 that the final point about Cavendish circulating erroneously dated 'offprints' was added in the proof stage.

64 Preface, *Correspondence*, p. ii.

65 Ibid., p. iii.

66 Ibid., p. vi.

father's papers. Watt Jr reported being immediately struck that the correspondence contained proofs of his father's claim 'ample, satisfactory, and conclusive'.[67] Nevertheless, he repeatedly sought the opinions of others. Almost immediately Watt Jr showed the papers to John Corrie, then President of the Philosophical Society of Birmingham, whose view of their significance coincided with his own. The next to see the correspondence, or at least extracts from it, in 1820 was Watt Jr's old Manchester friend Dr William Henry, who agreed with his assessment of the import of the materials. Henry saw the original correspondence later, in 1835 and 1836, on visits to Aston Hall. According to Watt Jr, this 'had the natural effect of strengthening the opinion he had formed and expressed in 1820; and upon the latter occasion he mentioned his intention of writing a history of Chemistry, in which he said he should do justice to my father's claims to the priority'.[68] On a visit to Edinburgh not long after his father's death, Watt consulted Drs Hope and Brewster about the correspondence, but they were unable to draw the same conclusions in favour of Watt's priority.

In September 1824, Sir Humphry Davy visited Watt Jr for a few days at Aston Hall and was shown the memoir for the *Britannica* and also the original correspondence on the water question. Watt Jr recalled the occasion thus:

> I directed his attention to what is there said [in the memoir] on my father's claim to the discovery of the theory of the composition of water; but the facts stated appeared to be new to him … . I mentioned my desire to do justice and inquired if he knew of any papers left by Mr Cavendish from which the date of his <u>conclusions</u> might be ascertained; but he was ignorant of the existence of any such papers. I then laid before him the press copies of my father's letters, and the original ones of his correspondents, which he read over with much interest, and appeared exceedingly struck with their contents. He expressed concern at the effect which their publication must produce (a concern not unnaturally proceeding from his known attachment to Mr Cavendish) and he did not then, or at our subsequent meeting in 1826, endeavour to lessen their force, or to call in question the deductions resulting from their perusal. In the last conversation I had with him here on the subject, he said he thought that my father's theory, admitting the latent heat, would prove correct.[69]

Watt Jr's mention of his 'subsequent meeting' with Davy in 1826 turns out to have been a very interesting occasion. On 30 August 1826 a meeting was held in the Assembly Rooms in Greenock to consider the erection of a monument to the memory of Watt. The local worthies were gathered to consider the appropriate form of commemoration. The meeting was informed that Watt Jr was in town and had expressed his preference for a statue from the chisel of Chantrey. After some discussion, a deputation was sent to bring important guests to the Meeting – Watt Jr, but also Sir Humphry Davy.

The Chairman, Sir M.S. Stewart, spoke first and it became plain that his ear had been bent by Watt Jr about his father's philosophical credentials. In the midst of a long panegyric are these words:

[67] Ibid., p. iv.
[68] Ibid., pp. iv, v and note.
[69] Ibid., pp. ix–x.

> I speak it under the correction of the high authority near me, that we greatly injure the fame and narrow the reputation of Mr Watt, if we consider him only as a great practical mechanic. I believe him to have been a profound philosopher, and a subtle chemist; and that it was by the aid and complete mastery of these sciences that his penetrating intellect, after years of intense labour, brought to perfection a series of combinations unexampled in the history of the world … bestowing a new and inexhaustible power upon civilized man.[70]

Watt Jr then rose and, after long preliminaries, announced that he would donate a sum of £2000 to be used in erecting a building for a library in which the statue would be placed. Davy also spoke in rhapsodic terms about Watt and 'the triumph of philosophy, as applied to practical purposes'.[71] The rhetorical force of presenting Davy as convinced by the water correspondence was considerable, not least because Davy's obituary of Cavendish and his *Elements of Chemistry* were major sources of the mythology surrounding that great natural philosopher. Davy's characterizations of Cavendish and his work were already standard reference points.

Arago was the next to be shown the original correspondence at Aston Hall, where he briefly sojourned *en route* to the British Association meeting at Edinburgh in 1834. It was already known that Arago was preparing his *Eloge* of Watt for the Académie des Sciences. We examine the springs of Arago's work and the saga of Watt Jr's exchanges with him in Chapter 6. For the moment we need only note what Watt Jr had to say about Arago and the water correspondence. He stated that during the 1834 visit he asked Arago if he had examined the water question, and that Arago responded positively, saying that he had convinced himself of Watt's priority by examining the available public documents. Watt Jr then showed Arago the correspondence which 'put the seal on his conviction'.[72] Arago asked to use the material and Watt Jr agreed. Watt Jr emphasized that the account of the water question given in the *Eloge* as read to the Institute in December 1834 'experienced no alteration' between then and the publication of the *Eloge* in 1839. The implication once again was that the correspondence provided a kernel of clear and enduring factual information about the water question.

Watt Jr recalled that in the same year as Arago's visit, Lord Brougham was writing the inscription for the monument to Watt in Westminster Abbey. As we have seen, Watt Jr called Brougham's attention to the water question, considering that the inscription might include something about Watt's priority. In the end it did not, but Brougham's perusal of the water correspondence started him upon a train of research. Watt Jr had asked Brougham to examine the available documents 'with the discrimination of a lawyer, and the impartiality of a judge'.[73] This led Brougham to investigations among the Cavendish papers and the archives of the Royal Society

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[70] 'Report from the Greenock Advertiser, of the Proceedings of a Meeting, held in the Assembly-Rooms, Greenock, for the purpose of deliberating on the erection of a Monument to the Memory of the late James Watt, Esquire', reproduced as an Appendix in George Williamson, *Letters Respecting the Watt Family*, 1840, pp. 50–67, at p. 54.

[71] Ibid., p. 59. Davy had evidently been recruited to the cause.

[72] Ibid., p. x.

[73] Ibid., p. xi.

that produced the story told in his ‘Historical Note’ appended to Arago’s *Eloge*. That ‘Historical Note’ itself carried further notes upon it by Watt Jr.

We have seen, then, that Watt Jr regarded the water correspondence as irrefutable factual evidence for his father’s priority. However, he continually sought reinforcement of that belief by revealing the correspondence to other key individuals. The water controversy assumed such importance in the filial project because it was the issue upon which Watt’s claims to philosophical eminence were challenged. They were challenged precisely because they were claims to philosophical status that Watt Jr and others considered crucial in presenting the great engineer as simultaneously the great student of fundamental nature. At stake in the end was the perceived relationship between science and technology.

Conclusion

We will meet Watt Jr many more times in the course of this study. We have seen enough, however, to understand the dimensions of his filial project and how his early life informed and impelled it. We have also seen that Watt Jr’s approach to the issue of priority of discovery was an empirical one, itself very much in tune with the pragmatic keeping of account that underlay his important contributions to the business that his father had founded. In this sense, keeping account was a major impetus to the water controversy. This empirical approach was not entirely personal. It drew strength from a philosophical stance that some other parties to the water question shared, including François Arago. Why did this prominent French scientist participate in the water controversy? To that question we now turn.

Chapter 6

The French Connection: Arago Re-opens the Controversy

Introduction

The *éloge*[1] of James Watt that was delivered to the Académie des Sciences by François Arago on 8 December 1834 and published nearly five years later is a fascinating document. It played a key role in the water controversy, setting out in very stark and challenging terms the case for Watt's priority and accusing Cavendish and his friends of underhand dealings. Being the occasion for Harcourt's counter-attack at the meeting of the BAAS in Birmingham in 1839, the *Eloge* became the hinge about which the controversy turned.

Why did Arago write the *Eloge* of Watt? Why did he make an issue of the water controversy in the way that he did? In France, Arago's treatment of Watt had major local implications that were quite different from those driving the controversy in Britain. I argue that the *Eloge* was one vehicle by which Arago sought to convey messages about the processes of invention and innovation and their importance in the industrial and political development of France. I suggest that his account of Watt also reflected to some extent Arago's concern to remodel and reform the communication processes of the Académie des Sciences. In addressing historical issues of discovery, priority and communication Arago intended that morals be drawn about the best way to organize the Académie's contemporary affairs. It is indeed ironic that an *Eloge* of a man who died in 1819, that was not begun until 1834, and which had timely publication as one of its subtexts, should have finally appeared in print in 1839 after the lengthy saga we now examine.

The first manifestation of the *Eloge* was the document prepared by Arago for delivery at the meeting of the Académie des Sciences in December 1834. This spoken version was important to Arago because it was delivered directly to a fashionable and powerful French audience. We can be reasonably sure that this version concentrated much more than the published one on Arago's 'dissertations' (as Watt Jr called them) on steam power in France and on open communication in science. Thus it probably contained less material than later published accounts on Watt as such and more on the messages that Arago sought to convey through his account of the life. This document is not available, but we can know something of its contents from two sources: press reports of the meeting, and exchanges between Arago and James Watt Jr during the course of preparing the spoken version of the

[1] I use *éloge* to refer to the spoken version of Arago's memoir of Watt, and *Eloge* to refer to the published versions.

éloge. On the way to the British Association meeting in Edinburgh in 1834 Arago met with Watt Jr. During Arago's stay in Britain, and in the months leading up to the delivery of the *éloge* later in the year, they were in regular contact.

When the *éloge* had been delivered, attention turned to its publication. This was to be a long and tortuous process in which Arago again maintained close contact with Watt Jr and also with other interested parties, notably Henry, Lord Brougham. Brougham had long been a defender and promoter of the reputation of James Watt. He was present at the Académie for the delivery of the *éloge* and he was to write crucial material appended to the published version as well as give advice on the larger text. Thus in the late 1830s numerous exchanges about the content of the *Eloge* occurred and we have archival records of James Watt Jr's comments and annotations upon an intermediate version and upon the first proofs of the document. These processes fed into the publication of the *Eloge* in France in 1839.[2]

The *Eloge*, Arago's Career and Concerns

Why did Arago write the *Eloge* of Watt? A straightforward answer might be that it was Arago's job as Perpetual Secretary of the Academy to prepare *éloges* of deceased members (Watt had been elected in 1808). Perhaps in writing about Watt Arago simply did his duty. However, Arago did not write all the *éloges* that were presented to the Academy. He chose those whom he was to write about and his biographers tell us that Arago used the *éloges* to convey messages about his vision of the relationship of science and society in industrialization. Here is John Cawood on the issue:

> Pendant la période où il fut à la fois Secrétaire perpétuel et député, les efforts d'Arago pour démontrer la fonction sociale de la science prirent trois formes essentielles. Dans ses écrits et dans ses discours à l'Académie et à la chambre des députés, tout d'abord, il développa un système général d'idées sur la rôle de la science dans le progrès social. Deuxièmement, il plaida pour que le développement industriel profite de l'application de la technologie. Il se préoccupa, enfin, des problèmes sociaux, éducatifs et politiques posés par le développement industriel. A l'Académie, il exprimait souvent ses idées dans les 'éloges', courtes biographies d'hommes de science distingués, qu'Arago transformait en déclarations générales sur la fonction de la science.[3]

[2] The *Eloge* appeared in France as: D.F.J. Arago, 'Eloge historique de James Watt', *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, **17**, 1840, lxi–clxxxviii, having also previously appeared in the *Annuaire du Bureau des Longitudes* for 1839.

[3] John Cawood, 'François Arago, homme de science et homme politique', *La Recherche*, **16**, 1985, 1469. On the *éloges* of Arago see Maurice Crosland, *Science under Control: The French Academy of Sciences 1795–1914*, 1992, pp. 360–61, and on those of Cuvier see Dorinda Outram, 'The language of natural power: The *Eloges* of George Cuvier', *History of Science*, **16**, 1978, 153–78. Translation of quotation: 'During the period when he held the position of perpetual secretary and deputy, Arago's efforts to show the social function of science took three essential forms. In his writings and his speeches at the Academy and at the Chamber of Deputies, from the first, he developed a general system of ideas on the role of science in social progress. Secondly, he argued that industrial development profits from the application of technology. He preoccupied himself, finally, with social, educational and political problems posed by industrial development. At the Academy he often put forward these

Arago praised the ideas of Condorcet that science was the way to the perfectibility of man, that science would dissipate prejudice and moral and intellectual sickness. The ideological benefits of science were complemented by its practical benefits through technology. It was in this connection that Arago discussed the steam engine 'qui, selon lui, avait bénéficié du développement des théories de la chaleur'.[4] His *éloge* of Watt devoted considerable space to an argument about the industrial gains to be had by application of steam power to industry and transportation. Arago's notable advocacy of Watt's priority in discovery of the composition of water was thus part of a larger picture in which he sought to assimilate science, technology and industrial development. Arago's more general political aims of expanding democratic franchise and of bringing education to the workers were readily united into this picture.

Arago was in many respects an heir to the industrializing ideology of J.-A Chaptal, who sought to advance the French economy by bringing science, engineering, entrepreneurship and a well-educated workforce together.[5] Horn and Jacob find many similarities between the views of Chaptal and Watt and an ongoing circle of mutual admiration including them both, especially via Berthollet. Crosland notes the similarities between the careers of Chaptal and Gay-Lussac, a point that I would echo for Gay-Lussac's good friend and coadjutor Arago. Arago and Chaptal were, of course, both members of the Society of Arcueil, though of different generations.[6] So Arago's views about the relations of science and industry were not unique to him but he was part of a tradition of thought in late eighteenth- and early nineteenth-century France.

In writing *éloges*, Arago was also coming to terms with a long tradition within the Académie. His predecessor, Georges Cuvier, managed a number of tropes in his *éloges*. The tendency of the natural philosopher to solitude and disengagement from the world was emphasized by Cuvier, but he also dealt with the dangers of 'solitude' transforming into 'isolation'. Cuvier's management of images of the natural philosopher was closely dependent upon the positions that he had chosen to take within the changing political climate of French society.[7] Arago faced quite other circumstances and took a very different personal political stance. In fact, Arago led a liberal political faction within the Académie directly opposed to Cuvier's attitudes and policies. The two were in open conflict about public access to the workings of the Académie and also ranged on opposite sides in the Cuvier–Geoffroy debate. Indeed, the latter debate was the occasion for provocative action by Arago, very soon after his 1830 election as Perpetual Secretary, to open the proceedings of the Académie to public scrutiny.[8]

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ideas in the *éloges*, short biographies of distinguished men of science, which Arago transformed into general declarations on the function of science.'

4 Cawood, 'François Arago', 1470.

5 See Jeff Horn and Margaret C. Jacob, 'Jean-Antoine Chaptal and the cultural roots of French industrialization', *Technology and Culture*, **39**, 1998, 671–98.

6 See Maurice Crosland, *The Society of Arcueil*, 1967, pp. 113–16, 247 and Crosland, *Gay-Lussac, Scientist and Bourgeois*, 1978.

7 Outram, 'The language of natural power', 153–178.

8 See Dorinda Outram, *Georges Cuvier*, 1987 and Toby A. Appel, *The Cuvier–Geoffroy Debate. French Biology in the Decades before Darwin*, 1987, pp. 120–21, 160–61.

6.1 François Arago

By 1834 Arago was approaching the height of his powers. As a young man he had risen quickly via the Ecole Polytechnique, a position at the Paris Observatory and election to the Académie des Sciences at the age of twenty-three. Though patronized and befriended by members of the Society of Arcueil, Arago's relationship with many of his colleagues was often tense. For example, he worked closely with J.B. Biot at various times but they were also continually in dispute. Arago's republican views led him into a political career from 1830. Indeed, his scientific and political careers were not really separate since as Perpetual Secretary of the Académie he pursued a political programme involving, among other things, opening up the scientific community to greater public access and the promotion of technological change. To the end of his life Arago continued to bring technological matters before the Académie. In 1852, a year before his death, he exhibited a sample of the new undersea telegraph cable laid, joining England and France.[9] In

his career in the Chamber of Deputies he frequently concerned himself with scientific and technological issues, promoting similar objectives. It is not surprising, then, that Arago, in his *éloges*, was much more anxious to stress the active involvement of scientific figures in practical and political affairs than was Cuvier. Arago also emphasized more than did Cuvier the dangers of solitude translating into isolation for the man of knowledge. For Arago, active communication was vital to the proper pursuit of the life of science.

In entering into issues of discovery and priority as Arago did in the *Eloge* of Watt, he was addressing matters that had been an abiding concern for him throughout his career. His early years as a young researcher in cooperation and competition with Biot had given Arago a strong sense of the importance of guarding and establishing priority. This issue recurred through Arago's career in a variety of connections: the disputes over the wave theory of light; Arago's support for Daguerre as the inventor of 'photography'; his support of Leverrier against the claims of Adams to the discovery of the planet Neptune. The positions on priority and discovery, what they consist in and how they are to be established and accounted for, were thus not something only discussed in the *Eloge* of Watt. They were an ongoing concern. What Arago had to say about Watt was conditioned by his prior writings about discovery and priority as generic issues. A point that emerges in all these examples as dear to Arago's heart (and important to his ideology) is that discovery and priority claims can and should be judged on the basis of *public* statements and claims, and must involve openness of communication. Arago believed that it was the scientist's responsibility to openly communicate findings. Not to do so was, in his view, an abdication of responsibility and a neglect of public duty. In consequence, if findings were not fully and openly communicated there could be no priority or credit claimed for them. This view was allied to Arago's republican doctrine. The founding of the *Comptes rendus* and other steps that Arago took to open up the Académie des Sciences to public scrutiny and to improve its publication practices were closely linked to his ideas about the appropriate mechanisms for crediting discovery and priority. Thus, before and after dealing with Watt, Arago relied heavily on priority of communication as a criterion of discovery.

In taking Daguerre's part against the Englishman W. Henry Fox-Talbot in the case of the discovery of photography, Arago tried to ensure the early communication of Daguerre's process through the Académie with his announcement on 7 January 1839. However, because the French government's pension to Daguerre was not secured until later that year, Arago's announcement on behalf of Daguerre had to be made in 'deliberately vague terms' and not fully published until August 1839.[10] Thus even where direct commercial concerns intruded, open communication should, according to Arago, be pursued as much as possible.

In 1843 Arago remarked on this question again, this time in some observations on the career of William Herschel. Arago was impressed by Herschel's habit of publishing his findings immediately. Herschel was to be contrasted in his openness with the researcher who, 'in love with his discoveries, as the miser is with his

[9] See John L. Davis, 'Artisans and savants: The role of the Academy of Sciences in the process of electrical innovation in France, 1850–1880', *Annals of Science*, **55**, 1998, 299.

[10] Crosland, *Science under Control*, p. 255.

treasure, buries them in the ground, takes care even lest his discoveries be suspected, for fear that some other experimenter develops them or applies them. The public owes nothing to someone who has rendered no service to it.'[11] As Crosland observes, Arago's emphasis was upon avoiding priority disputes by clearly dated publication (as in the *Comptes rendus*). Retrospective claims based on testimony could never be authentic in the way that such publicly documented announcements were.[12] Once again, in October 1846, during a speech at the Académie supporting Leverrier against the recently announced, but unpublished, claims of John Couch Adams in the dispute over the discovery of Neptune, Arago invoked the criterion of open communication:

> What! M. Le Verrier has made his research available to the entire scientific world: following the formulae of our learned compatriot, everyone has been able to see the new planet … and to-day we are called upon to share this glory, so loyally and legitimately acquired, with a young man who has communicated nothing to the public … [13]

Given this preoccupation with priority through open communication, it is hardly surprising that Arago sided with Watt in the water controversy case. For Arago the emergent characterization of Cavendish as reluctant to publish, and not needful of fame because of his aristocratic and financial status, would automatically incline him against Cavendish's case. The fact that Cuvier had been Cavendish's elogist did not help matters. If Watt could claim priority of communication, then the case was made. Private testimony concerning Cavendish's actions, theories about what he must have thought, or testimonies to his character were beside the point. Also corruption of the communication process of the kind that Arago, and Brougham, claimed had gone on through the wiles of Blagden and Cavendish was anathema. We must not forget, finally, that Arago was likely to welcome the chance to expose, or at least give an airing to, such '*ancien régime*' corruption.

Thus the criteria of discovery that Arago sought to apply in the water controversy emphasized precisely those aspects of scientific communication that he was working hard to facilitate through his role in the scientific community. This provides, then, a major contextual reason why Arago would prefer one sort of accounting of discovery over another. He, along with the rest of the Watt camp, emphasized the importance of the dates of the key documents in the water controversy. The Cavendish camp on the contrary perforce went in for complex hermeneutics, what Watt Jr called 'tedious sophistical declamation upon the merits of … their theories'.[14]

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[11] *Comptes rendus*, **17**, 1843, 776n, taken from Arago, 'Notice sur la vie et les travaux de William Herschel', *Annuaire du Bureau des Longitudes*, 1842, 462–63, as quoted in Crosland, *Science under Control*, p. 280. Arago, *Biographies of Distinguished Scientific Men*, 1857, 195 remarks on William Herschel's memoirs on Newton's rings that Herschel himself had 'said that it was the only occasion on which he had reason to regret having, according to his constant method, published his labours immediately, as fast as they were performed'.

[12] Crosland, *Science under Control*, p. 280.

[13] Ibid., p. 372. See also Morton Grosser, *The Discovery of Neptune*, 1962 and Robert W. Smith, 'The Cambridge Network in action: The discovery of Neptune', *Isis*, **80**, 1989, 395–422.

[14] Watt Jr, 'Note upon the Presidents discourse at the Birmingham Scientific Meeting, for Eloge p.

We can say that Arago's *Eloge* of Watt had a special significance for him because of the opportunities it offered to develop themes close to his heart and central to his public career as scientist and politician. The time that he devoted to the *Eloge*, the constant delays in order to accommodate Watt Jr (of which more below), are indications of his concern to back his case as authentically and effectively as possible.

Gathering Material for the *Eloge*: Arago and James Watt Jr

In late August and early September 1834 Arago visited Britain to attend the British Association meeting in Edinburgh. Whewell and William Buckland, the geologist and palaeontologist, had tried to persuade Arago to attend the Cambridge gathering of the Association in 1833 but those plans had fallen through. All were delighted when it became clear that Arago would attend in 1834. It was during the course of the 1834 visit that Arago had an important meeting with James Watt Jr preparatory to writing the *éloge* of Watt.

Arago's intention to visit was announced in a rather roundabout fashion. Watt Jr's friend, the sculptor Francis Chantrey, wrote to him on 20 August 1834 enclosing a letter from Joseph Barclay Pentland to the Reverend William Buckland that had been forwarded to him (Chantrey) by Buckland. Pentland had been an intermediary between French and British scientists since his days in the early 1820s as an assistant in Cuvier's laboratory. A former student of Buckland, and the chief intermediary between him and Cuvier in matters palaeontological, by 1834 Pentland appears to have been functioning as Arago's 'minder' in the great astronomer's relations with the British.[15] Pentland's letter announced that he had persuaded Arago to accompany him to the Edinburgh meeting and that Arago wished to visit Buckland in Oxford and Watt Jr in Birmingham.

> Arago intends to visit Birmingham to see your friend Mr Watt, as he proposes to read his Eloge of the Illustrious Inventor of the Steam Engine, at the next Publick meeting of the Institute in November and will be glad to talk over many circumstances with your friend & Mr Bolton, as well as to visit their superb Establishment. Will you therefore write a line to Mr Watt on the subject.[16]

90', Muirhead Papers, MS GEN 1354/296. This note, modified by Muirhead, subsequently appeared in his translation of the *Eloge*, pp. 115–17.

[15] On Pentland see William A.S. Sarjeant and Justin B. Delair, 'An Irish naturalist in Cuvier's laboratory. The letters of Joseph Pentland 1820–1832', *Bulletin of the British Museum of Natural History (Historical Series)*, **6**, 1980, 245–319. Pentland had close relations with the Foreign Office. He undertook diplomatic representative tasks in South America in the later 1830s. Through most of the exchanges that we are concerned with his mail travelled via diplomatic pouches and the Foreign Office. The possibility occurs that Pentland might have been 'keeping an eye' on the situation in France.

[16] J.B. Pentland to William Buckland, 15 August 1834, Watt Papers (Doldowlod), W/12. At this stage Arago planned to read the *éloge* at the November meeting of the Académie but the reading was eventually postponed to December.

Arago's wish to visit the Soho works, and the inclusion of major industrial centres in his itinerary, show that he was anxious to gain more first-hand information about British industrialization. The proposed visit to Soho was a sensitive matter because of problems of industrial espionage. Watt Jr explained that visitors could not be shown around the works for that reason and that the rule was applied without exception.[17] Watt Jr was anxious to spend two days with Arago so as to be able to convey 'all the information he ought to possess respecting the subject of his Eloge'.[18]

In the event Arago was able to spend only one day at Watt Jr's house, Monday, 1 September. As Watt Jr later recalled to George Rennie:

> [Arago] was in such a hurry to get to the Edinburgh Meeting that I could only get him to give me one day, during which I communicated all that time permitted, and above all completely satisfied him by an inspection of my fathers private correspondence that he had had foul play between Cavendish & his friends, with regard to the priority of publication.[19]

There was hope of a second visit from Arago after the Edinburgh meeting but, as Watt Jr recalled, instead of returning to Birmingham, Arago 'spent his time in Dumfriesshire and then hurried to London in bad health'.[20] From London, Arago expressed his regret at not being able to return to Aston Hall. Arago hoped for, and relied upon, Watt Jr's continuing assistance. In particular, he said: 'J'espère que vous voudrez bien remarquer <u>qu'il</u> <u>n'est</u> <u>plus</u> <u>en</u> <u>mon</u> <u>pouvoir</u> de ne pas traiter en détail la question de priorité que la découverte de la composition de l'eau a fait naître, et que vous seul pouvez ma conduire à une solution définitive.'[21] Further, Arago reassured Watt Jr that he would use the correspondence shown to him with 'toute la réserve que le grand nom de Cavendish commande'. It is clear that at this stage there was certainly no intention to inflame matters with the Cavendish family unnecessarily. Arago also advised that he had seen David Brewster, who had said nothing to change the view of the matter.[22]

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[17] Watt Jr to William Buckland, 26 August 1834, Watt Papers (Doldowlod), W/12. Such caution on the part of individual establishments was matched by government measures, though the latter were largely ineffective. See David J. Jeremy, 'Damming the flood: British government efforts to check the outflow of technicians and machinery, 1780–1843', *Business History Review*, **51**, 1977, 1–34.

[18] Watt Jr to Chantrey, 22 August 1834; Buckland to Chantrey, 20 August 1834; Watt Jr to Buckland, 26 August 1834, Watt Papers (Doldowlod), W/12.

[19] Watt Jr to George Rennie, 24 October 1834, Watt Papers (Doldowlod), W/12. Watt Jr continued: 'Indeed he appeared to have made up his mind upon this subject from the published documents alone, before seeing those I possess, of which I have given him a copy.'

[20] Ibid.

[21] Arago to Watt Jr, 22 September 1834, Watt Papers (Doldowlod), W/10.

[22] Ibid. The reference is to those passages in his correspondence in which Watt wrote defiantly about not being subjugated by the might of the House of Cavendish. Brewster had written the article 'Watt' for his *Edinburgh Encyclopaedia*. Although he quoted there extensively from Watt Jr's account of the water controversy, Brewster also clearly stated his belief that Watt's claim was not strong. See 'James Watt', *The Edinburgh Encyclopaedia*, 18 vols, 1830, vol. 18, pp. 784–87. On Brewster's changing views on the water controversy see below, Chapter 7.

Watt Jr dutifully sent a flurry of documents to London after Arago, some via Peter Ewart, whom Watt Jr had invited to Aston Hall so as to meet Arago on his anticipated return there from Edinburgh. Ewart was one of Watt's few surviving pupils, and Watt Jr was anxious that Arago learn from him about his father's skills as a craftsman. Ewart and Arago did meet and discuss this question in London and as late as 2 October there were plans for Arago to return to Birmingham with Ewart for another meeting with Watt Jr Arago's indisposition prevented this.[23] When it was clear that Arago was not going to visit him again, Watt Jr wrote him a series of long letters (on 3 October, 8 October and 13 October 1834) dealing with various aspects of his father's life. As Watt Jr recalled to George Rennie: '[Arago] has kept me hard at work for several weeks; much of which would have been avoided had he kept his appointment for returning here. But your men of Genius will take their own course, and I am satisfied that neither zeal nor talent will be wanting on his part and I only fear he has allowed himself too little time.'[24]

Watt Jr was particularly anxious that Arago take seriously his father's 'abilities as a draftsman and as a workman'. Ewart testified to these abilities and Watt Jr's effort to bring Ewart and Arago together seems to have been motivated primarily by the desire to convey that message. Watt Jr inundated Arago with drawings made by his father both early and late in his life: 'There was no process, or manipulation of Art, with which he was unacquainted, and for which he did not occasionally practice either to supply his own wants, or to endeavour to perfect them. In fact, this was one of the most distinguishing parts of his character.' Watt Jr went to almost the same lengths to convince Arago of the success that his father had enjoyed as a civil engineer. He devoted one letter (13 October 1834) to recounting the defences that Boulton & Watt made of their patent in which he made some play of the characteristics of the witnesses on either side, his father having scientific support and the opposition supported largely by engine makers. Boulton & Watt's counsel accused the latter, Watt Jr said, of coming to the court 'to prostitute their own ignorance'. When the case was won, we are told, Boulton & Watt showed gentlemanly restraint in recovering the arrears of payments due to them: 'To those who had continued to pay, they made large and liberal allowances, and they exacted far short of their dues from their opponents.'[25]

The Construction of the *Eloge*

The *éloge* as read in early December 1834 remains something of a mystery. I have not found the copied manuscript version promised to James Watt Jr shortly after the reading. But we can make an educated guess as to its content in various ways. We know something of what was added to the *éloge* later, and so by a process of subtraction from the published version we can mock up the orally delivered one. We know what Watt Jr had supplied to Arago in 1834 in his letters and by way of

23 See Ewart to Watt Jr, 2 October 1834, Watt Papers, W/10 and Watt to Arago, [3 October] 1834, Watt Papers, WP/10.

24 Watt Jr to Rennie, 24 October 1834, Watt Papers, W/12.

25 Watt Jr to Arago, 3 October? 1834, Watt Papers, WP/10.

other documents. The *éloge* was also reviewed quite extensively in the French press, there being accounts in the following: *Courier Français*, *Journal de Commerce*, *Gazette de France*, *Journal de Paris*, *Journal des Débats* and *Temps*.[26] These accounts are of some assistance. Pentland's letters tell us that the *éloge* as read was watered down in technical detail because of the popular nature of the audience. Arago stated that he wanted to include more material on the water controversy in the published version because some of the Academicians were not convinced of Watt's priority on the strength of what he had said.

From these sources we can conclude that the *éloge* of Watt was transformed while it was 'in press'. It was in such a state for a very long time, much longer than was initially intended. Correspondence from Pentland in the aftermath of the reading discussed publication as a matter of months or up to a year away. The *éloge* was repeatedly described as 'in press' in correspondence over ensuing years. Many reasons might be invoked for this long delay. In one sense it was an institutional habit – the Académie was noted for tardiness in the publication of its *Memoirs*. However, Arago was trying to address that problem. Another reason for the delay was the pressure of business for Arago during these years in all his roles within the Académie and in the Chamber of Deputies, as also in metropolitan Paris politics. Watt Jr was also preoccupied at various times with his business affairs, the retirement of his partner, Matthew Robinson Boulton, and eventually extracting himself from the business. Land disputes on his Radnorshire estates distracted Watt Jr, as did the threatened intrusion of railway projects upon them. However, at least some of the delay must be attributed to the perceived necessity (variously on Arago's part and on that of Watt Jr and Brougham) for wholesale changes and additions to the text.

The reaction of the French public press and from within the Académie prompted Arago to try to shore up some of his arguments. Beyond this, however, Arago had promised to consult Watt Jr before publishing anything. Brougham also was treated as a trusted adviser. This meant that between 1834 and 1839 there was much epistolary to-and-fro. More than this, on at least two occasions Watt Jr visited Paris primarily for interviews and negotiations with Arago about the *Eloge*.

Once it became clear that Arago would not visit Aston Hall again before returning to Paris, Watt Jr set about transcribing documents and sending them to him. The first batch, sent with Peter Ewart, included: transcripts of correspondence relating to the decomposition of water together with a translation of Blagden's paper in Crell's *Annals* of 1786; Watt's specifications for a range of mechanical inventions; two manuscript volumes of Watt's reports as a civil engineer; Dr Henry's letter of 8 June 1820; Hatchette's 'Notice des Travaux de Mr Watt' of 11 August 1819; and two portraits of Watt, one for Arago and the other for Pentland.[27] Shortly afterwards, Watt Jr sent the first of a sequence of long letters to Arago concerning Watt. At this stage, too, the agreement seems to have been struck that Watt Jr would see the *Eloge*

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[26] This according to the reports of J.B. Pentland, who was monitoring the French press and promised to send Watt Jr copies of mentions of the *éloge*.

[27] 'List of Documents sent by Mr Ewart to Mr Arago', 1 October 1834, Watt Papers (Doldowlod), W/10. The transcripts of the water controversy correspondence were annotated as to be left with Arago. The rest of the material, apart from the portraits, was presumably to be returned. The list was signed by Ewart and so perhaps functioned as a receipt.

before it was printed. The rationale was that Arago, working necessarily from imperfect sources, would fall into errors of omission and commission that Watt Jr, with his superior documentary base, would be in a position to correct.[28]

For material relating to 'the leading events of my father's life' Watt Jr pointed Arago to his own short memoir in the *Supplement* to the *Encyclopaedia Britannica* and to Dr Robison's essay on steam and steam engines with Watt's own notes thereon. Watt Jr wrote proudly of his father's abilities as a draftsman and workman, a point that he was anxious to make. He also conveyed the admiration that Rennie, Telford and Smeaton had had for his father's work as a civil engineer. Arago was referred to Robison's edition of Joseph Black's lectures for material on how Watt had conducted experiments supporting Black's development of the Theory of Latent Heat.[29]

On 8 October Watt Jr sent to Arago a list of English publications in which the water question was treated, pronouncing on the value of each one. Watt Jr singled out particularly the *Encyclopaedia Britannica* article 'Water' (in what he wrongly identified as the 'Ist edn 1797') as a 'very elaborate & well written article [which] states the claims of Mr Watt, Cavendish, Priestley & Lavoisier fairly giving Mr Watt the merit of the discovery of the Theory'.[30] There followed on 13 October a long letter from Watt Jr, already referred to, recounting Boulton & Watt's court battles. Watt also sent the Minutes of the Committee of the House of Commons, and a printed pamphlet of the arguments of the judges in the Common Pleas and Kings Bench containing the Patent of 1769, the Act of Parliament in 1775 and other material.[31]

These seem to have been the main communications before the initial composition and reading of the *éloge*, Arago saying, via Pentland, that he had documents enough.[32] Arago, of course, had other sources. Leonard Horner had promised him information on the numbers of steam engines employed in factories in Britain. John Taylor was obtaining similar information about the mines of Cornwall. Arago had visited key sites in Greenock, Watt's birthplace, and had met several friends of Watt who had given recollections of the great man. Chantrey gave advice on Watt's statue-copying machine and Faraday on the machinery for producing medals and coins that Watt had developed.[33] On this basis, and with whatever other sources he had available to him locally, Arago, who by his own testimony had not written a line of the *éloge* as of the end of September 1834, composed the piece delivered to the Académie on 8 December.

Watt Jr's access to, and therefore response to, the *éloge* as delivered was delayed. He received from Pentland copies of newspaper reports and summaries. Arago was intent upon beefing up the section on the water question. As Pentland reported, Arago wanted

[28] Watt Jr to Arago, 3 October?, 1834, Watt Papers, WP/10.

[29] Ibid.

[30] Watt Jr to Arago, 8 October 1834, Watt Papers (Doldowlod) W/10.

[31] Watt Jr reports having done this in Watt Jr to Peter Ewart, 24 October 1834, Watt Papers (Doldowlod), W/12.

[32] Reported in ibid.

[33] Arago to Watt Jr, 30 September 1834, Watt Papers (Doldowlod), W/10.

> to show the world the importance of this early part of Mr Watt's Scientifick Career and to prove on the authority of Priestley & others (wch he had neglected to do before ye publick reading of his Eloge) that your father preceded Cavendish & Lavoisier by several months in one of the most important discoveries of modern Chemistry.[34]

In the New Year 1835 Pentland was still promising Watt Jr a copy of the *éloge* soon, even as he explained how Arago's other engagements were delaying the revisions. Pentland did, however, send Watt Jr an extract 'drawn up at my suggestion, & under my eyes' from Arago's manuscript. He requested that Watt Jr send it to Professor Jameson of Edinburgh 'for his Journal'.[35] This was a reference to Robert Jameson, editor of the *Edinburgh New Philosophical Journal* in which Arago had already published a number of his *éloges*.

Watt Jr's watchful and critical eye went to work immediately. He found the newspaper reports of the *éloge* 'full of mistakes and blunders of all kinds'. The abstract of the memoir made by Pentland contained 'several erroneous statements which I am sure Mr Arago will thank me for pointing out and be glad to have the opportunity of correcting … '.[36] Watt Jr decided not to send the abstract to Jameson, 'as I am sure you will agree with me that nothing ought to appear in print, particularly in this country, in Mr Arago's name, until it has attained the utmost degree of accuracy'.[37] Watt Jr hoped that Arago would accompany Pentland to the Dublin meeting of the British Association in June 1835 and visit him on the way so that he could go through the *éloge* with Arago with all the sources before him. There was little hope of this, however, since Arago intended to visit his native town, Perpignan, on an electioneering tour.[38]

Watt Jr's liking for authentic documents was satisfied by a key acquisition that he quickly passed on to Arago. This was a memorandum sent to Watt Jr by his cousin Miss Jane Campbell, who in 1798 had taken down from her mother's dictation an account of Watt's early years. Miss Campbell's mother, Mrs Marion Campbell, was the great engineer's first cousin, a daughter of his mother's brother, Mr Muirhead. The cousins had spent much time together in their youth and on that basis Mrs Campbell had recorded her recollections. Watt Jr's advice to Arago was to 'obliterate what you have already written of this period of my father's life, and to subscribe, if you please, this entire narrative in its place'.[39] Arago acceded to Watt Jr's wishes, though he was unable to see any material difference between

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34 J.B. Pentland to J. Watt Jr, [15?] December 1834, Watt Papers (Doldowlod), W/10.

35 J.B. Pentland to Watt Jr, 3 January [1835], Watt Papers (Doldowlod), W/10.

36 Watt Jr to Pentland, 19 January 1835, Watt Papers (Doldowlod), W/10.

37 Ibid.

38 Ibid and J.B. Pentland to Watt Jr, 23 January 1835, Watt Papers (Doldowlod), W/10.

39 Watt Jr to Arago, 22 January 1835, Watt Papers (Doldowlod), W/10. See Eric Robinson, 'James Watt and the tea kettle. A myth justified', *History Today*, **6**, April 1956, 261–65. Robinson here misidentifies the document that Arago used. It was not Dr Gibson's letter but the memorandum discussed here. This document, via the account of it given in the *Eloge*, became the basis for two nineteenth-century paintings: Robert W. Buss, *Watt's First Experiment with Steam*, exhibited at the Royal Academy of Arts in 1845, and engraved by James Scott; and the better-known painting by Marcus Stone, *Watt discovering the Condensation of Steam*, 1863. See David Philip Miller, 'True Myths: James Watt's Kettle, his Condenser and his Chemistry', *History of Science*, forthcoming.

Mrs Campbell's account and what he had already written based on the testimony of others.[40]

The delay in getting a copy of the *éloge* to Watt Jr continued despite repeated promises. Pentland, in trying to explain Arago's dilatoriness, invoked his election to the Corporation of Paris and his work for the Chamber of Deputies, but also Arago's character as a person 'who with little natural order, is suddenly launched in a life of business, and of a business to which he has never been accustomed'.[41] Finally the promised copy of the *éloge* arrived at Aston Hall sometime in early June 1835, having been sent by Pentland with a letter of 6 June.[42] From that date the reasons, and apologies, for delay were issued from Birmingham rather than Paris.

By August, according to Pentland, Arago was 'extremely anxious' to hear from Watt Jr and was taking silence as displeasure. Watt Jr for his part offered the illness and death of a friend and relative, Dr Gibson, as the reason for not yet having given Arago's memoir more than a 'cursory perusal'.[43] Watt Jr planned to visit Paris later in the year with the specific and sole object of seeing Arago about the memoir. Pentland presented the matter as urgent: the time for printing the memoirs of the Institute was at hand, but Arago had promised not to publish until Watt Jr had had his say on the manuscript. In November 1835 Watt Jr explained his continuing failure to respond. His business partner, Matthew Robinson Boulton, was finally retiring and Watt Jr had decided to take the whole business upon himself.[44]

Watt Jr was very pleased with Arago's account of the water question. There were still errors to deal with, again especially in relation to Watt's early years. Watt Jr reiterated that he wanted Arago to 'adopt implicitly the narrative of Mrs Campbell' in preference to other 'traditionary stories'. However, beyond this no comprehensive response was forthcoming.[45] The next year Watt Jr was still promising to devote himself to the memoir, but Arago had ceased to communicate with him. Watt Jr now pleaded lawsuits against encroachers upon his Radnorshire estates as the excuse for literary inactivity. He asked if Arago could defer publication until spring, and pledged once more to give Arago his remarks by that time or else to deliver them personally to Paris.[46]

Watt Jr was in Paris by 21 March 1837 and over the next few weeks met Arago a number of times about the memoir. We have from that time a set of notes on the

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[40] Arago to Watt Jr, 16 February 1835, Watt Papers (Doldowlod), W/10.

[41] Pentland to Watt Jr, 17 April 1835, Watt Papers (Doldowlod), W/10.

[42] Pentland to Watt Jr, 6 June 1835; Watt Jr to Pentland, 14 June 1835, Watt Papers (Doldowlod), W/10.

[43] Pentland to Watt Jr, 13 August 1835; Watt Jr to Pentland, 29 August 1835, Watt Papers (Doldowlod), W/10.

[44] Watt Jr to Arago, 8 November 1835, W/10. Watt Jr did not become the sole owner of the business until a deed of dissolution of the co-partnership on 1 October 1840. On his partner's retirement and the rearrangement of the business see W.K.V. Gale, 'Soho Foundry: Some facts and fallacies', *Transactions of the Newcomen Society*, **34**, 1961–62, 83 and Robert B. Williams, 'Accounting for management as an expression of eighteenth century rationalism: Two case studies', unpublished PhD thesis, University of Wollongong, 1995, pp. 334–35.

[45] Watt Jr to Pentland, 8 November 1835, Watt Papers (Doldowlod), W/10.

[46] Watt Jr to Pentland, 30 December 1836, Watt Papers (Doldowlod), W/10.

memoir by Watt Jr.[47] They reveal that he was still preoccupied with family history, pointing out that Watt's father was not a shipbuilder, as Arago apparently had it, but a ships' chandler, general merchant and local worthy of Greenock. Watt Jr referred Arago yet again to the memorandum of Mrs Campbell for authentic information about Watt's early years. From Watt's letters to his father the story of the London 'apprenticeship' period was outlined. Watt Jr, in the notes, also argued Savery's position in the history of steam as the first to construct a steam engine and to apply it to practical purposes. On Papin, however, Watt Jr was almost silent. Given the contentiousness of Arago's claims for Papin, it seems that Watt Jr may have been diplomatically avoiding a fight when he stated simply that he offered 'no remarks upon Papin's contrivances as I have not been able to procure the books referred to, & have not entered into any examination of the evidence'.[48]

Watt Jr offered comments and information on a range of other topics that need not detain us beyond listing them.[49] Also buried in the notes is an interesting observation by Watt Jr on Edmund Burke's opposition to the 1775 Patent Extension Bill. Arago had sought to make political capital out of this supposed opposition. It was useful to Arago to depict conservative forces opposing technological ventures that subsequently turned out to be of enormous national economic value. Presenting Burke's opposition in this light would, he probably hoped, show up those of the legitimist party in France who opposed the encouragement of new technologies. Watt Jr's advice on what Burke's opposition signified would not have been welcome since it implied that the opposition was merely the outcome of attempted service to a constituent: 'Mr Burke opposed the bill at the instance of a Mr Gainsborough who claimed some similar invention. He did so as a matter of routine, usual between members and their constituents.'[50] Watt Jr subsequently observed that whilst he had been able to persuade Arago to tone down his remarks on Burke, he had not been successful in removing them altogether along with the political message: 'I could not get him to omit the part about Burke, although Brougham had also remonstrated upon it; but he has much altered it, and paid him some high comp^{ts}; as I said to him "couronnant de fleurs sa victime avant de l'immoler".'[51]

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47 'Notes upon Mr Arago's Memoir of Mr Watt. Paris April 1837', Watt Papers (Doldowlod), G/19.

48 Ibid.

49 These included: Matthew Boulton's status as a manufacturer and his relationship with Watt; those who assisted Watt in obtaining prolongation of the 1775 Patent; the fact that Boulton & Watt in their early days did not manufacture many of the parts for their steam engines; the witnesses who testified for Watt at various legal proceedings; Watt's involvement in improvements of his estates in Wales; his work on apparatus for Thomas Beddoes's pneumatic medicine ventures; the commemoration of Watt at Greenock by public subscription and by donation from Watt Jr to build a public library; the unfulfilled intention of government to recognize Watt's distinction by a peerage.

50 Ibid. Watt Jr's observation is corroborated by Burke's correspondence: Edmund Burke to Richard Champion, 28 December 1775, in George H. Guttridge (ed.), *The Correspondence of Edmund Burke*, vol. III, 1961, pp. 239–41 and Edmund Burke to Robert Smith, 6 April 1775 in R.B. McDowell and John A. Woods (eds), *The Correspondence of Edmund Burke*, vol. IX, 1970, pp. 406–408, at p. 407. Here Burke presents himself as the good local member simply doing the bidding of his Bristol constituents in relaying their opposition to Boulton & Watt's application.

51 J. Watt Jr to M.R. Boulton, 2 May 1839, Watt Papers (Doldowlod), W/10.

Watt Jr not only advised Arago to pay floral tribute to Burke before immolating him, but he also suggested that he add some remarks about changes to the patent law promoted by Lord Brougham. These changes were suggested on the first proofs of the *Eloge*. The final result read as follows. Referring to those who opposed the Boulton & Watt application for an extension of their patent, Arago stated:

> I was curious to learn to what class of society those members of Parliament belonged … who refused to the man of genius a small fraction of that wealth which he was about to create. Conceive my surprise, when I learned that at their head stood the celebrated Burke! Is it then the fact, that a man may be given to profound thought, may possess extensive knowledge and sterling honesty, be pre-eminently endowed with oratorical talents to move and carry along with him political assemblies, and yet be wanting in plain common sense? Since the important and wise improvements which Lord Brougham has introduced into the law of patents, inventors will not be subjected to that long series of annoyances to which Mr Watt was exposed.[52]

Thus Arago did take Watt Jr's advice up to a point, but the possibility of a stab at Burke was too tempting. We would be safe in assuming, I think, that Burke had been immolated in the original delivery of the *éloge* before the Académie in 1834 without the benefit of floral tribute. That would have been the opportunity that Arago needed. He could afford to compromise somewhat with the cultural requirements of Watt Jr and Brougham in their judgement of the best way to position the great engineer in the political climate of Britain in the late 1830s. We can consider more fully now the next stage, the production and correction of the first proofs of the *Eloge*.

After his return from Paris in the spring of 1837, Watt Jr wrote to Arago thanking him for his 'great civilities and attentions' during the visit. He was glad particularly for Arago's 'ready compliance with my wishes of rendering your eloge conformable to the facts supplied by me of the personal history of my father'.[53] Further additions to the *Eloge* were, however, afoot. As Watt Jr reported, Lord Brougham had completed his researches among the Cavendish papers and written his 'Historical Note' on Watt's claims to 'the first invention of the theory of the decomposition of Water'. Watt Jr had met with Brougham about the Note and they went together to the Royal Society where they inspected Cavendish's original manuscript of 'Experiments on air' and the interpolations that it contained, clearly in Blagden's hand. These interpolations appeared to show that Cavendish's paper had been surreptitiously altered in important ways to make it appear that he had clear priority over Watt.[54] Brougham had entrusted his memoir to Watt Jr for comments before it was forwarded to Arago. It appears to have already been agreed that Brougham's memoir would be appended to the *Eloge* as published. Watt Jr looked forward to the proof sheets.[55] Almost a year later, in April 1838, he was still waiting.

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52 The passage as translated in *Life of James Watt by M. Arago*, 1839, p. 55.

53 Watt Jr to Arago, 5 June 1837, Press Copy, Watt Papers (Doldowlod), W/10.

54 On these alterations see Henry Brougham, 'Historical note on the discovery of the theory of the composition of water', in François Arago, *Historical Eloge of James Watt … Translated … by James Patrick Muirhead*, 1839, pp. 157–73.

55 Watt Jr to Arago, 5 June 1837, Press Copy, Watt Papers (Doldowlod), W/10.

Finally, on 17 March 1839, Arago wrote to Watt Jr enclosing the first dozen sheets of the proofs with a promise of the rest to follow shortly and the request that Watt Jr return them as soon as possible.[56] Watt Jr's immediate response was favourable and he was gratified that Arago had incorporated the information about his father's personal history. However, it appeared to him that Arago had 'probably mislaid and forgotten' the 'additional Memorandums' that Watt Jr had left with Arago in Paris in 1837. Watt Jr repeated his view that incorporation of these suggested changes was necessary 'to make it altogether agreeable to my father's surviving friends and the descendants of those who are dead'.[57]

Watt Jr intended to return to Paris with the corrected proofs in order to provide Arago personally 'such further explanations and corroborative proofs as I am now able to furnish from other and unforeseen sources'. Watt Jr had other reasons for the journey. He was suffering from a complaint in his legs that had confined him to the house since the beginning of the year. He hoped that the journey would improve his condition.[58] A few days later correspondence with Pentland reveals that the latter was back on the scene after his absence in South America. Watt Jr looked forward to seeing Pentland again and asked him to keep Arago 'in humour until he sees me'.[59]

Some time before 19 April Watt Jr received the remaining proofs, for he wrote to Pentland that day thanking him for them and advising his intention to leave for Paris on the following Sunday, expecting to arrive four to six days later.[60] We learn from Pentland that he had persuaded Arago to wait. Pentland was anxious to see Watt Jr there because Brougham was still in Paris. The noble Lord had been giving notes about Watt to Arago 'which I should be anxious you shd see before they are printed since I am one of those who presumes to doubt his Ldp's infallibility in Science as in Politics'.[61]

Watt Jr arrived in Paris on the Saturday and had a meeting with Arago the following Monday. On the Wednesday Watt Jr's party and Pentland breakfasted with Arago at the Observatory and then Watt Jr and Arago retired for three or four hours' work. Arago adopted almost every one of Watt Jr's suggestions. It was at this point that the final struggle over the references to Burke occurred. Watt Jr confided to M.R. Boulton that other of Arago's 'dissertations I judged it useless to attempt to get him to suppress or abridge'.[62] A week later we get a glimpse of Watt Jr's still ongoing discussions with Arago, this time concerning Arkwright, his fortune and the development of spinning machinery. They were evidently extracting what they

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56 Arago to Watt Jr, 17 March 1839, Watt Papers (Doldowlod), W/10.

57 Watt Jr to Arago, 4 April 1839, Watt Papers (Doldowlod), W/10.

58 Ibid. Watt Jr advised that if the second batch of proofs had not arrived in the course of the next fortnight he would begin his journey then, meeting with Lord Brougham on his way through London.

59 Watt Jr to Pentland, 9 April 1839; Pentland to Watt Jr, 4 April [1839], Watt Papers (Doldowlod), W/10. Pentland's letter conveyed Arago's anxiety to publish almost immediately.

60 Watt Jr to Pentland, 19 April 1839, Watt Papers (Doldowlod), W/10. It is interesting to note that Watt Jr intended to stay again at Maurice's Hotel and that he required accommodation for himself, a friend and servants.

61 Pentland to Watt Jr, 16 April 1839, Watt Papers (Doldowlod), W/10.

62 Watt Jr to M.R. Boulton, 2 May 1839, Watt Papers (Doldowlod), W/10.

could from 'Mr Baines' Book' of value to Arago's account. Watt Jr was also proffering more information about his father's later years and his investment in the estate on the banks of the River Wye in Wales. Watt had engaged in various improvements to the estate in which his son had followed him.[63] More meetings occurred with Watt Jr having a seemingly endless fund of changes to suggest. Finally, he advised Arago that he had 'exhausted all I have to say', and showed that he had a sense of humour:

> I will not answer that if I should remain here a month longer, fresh matter may not occur to me. I therefore beg to suggest your adopting the expedient proposed respecting a Gentleman we talked about yesterday and applying forthwith to Louis Philippe for an order for my banishment from Paris.[64]

Even as the final printing proceeded, Watt Jr was sending corrections. But by mid-June all was over and attention was shifting to despatch of early copies of the *Eloge* to those who should have them.[65] The long saga of the production of the French version of the *Eloge* was at an end.

What were the key changes made in the *Eloge* over the years? One of the most important from our point of view was the addition of Brougham's 'Historical Note' on the water question as an appendix. Arago's account of the water controversy, although beefed up from the version read to the Académie, still used very little primary information. He quoted one or two key passages but did not exploit the Watt correspondence significantly. The inclusion of Brougham's note rectified that situation and arguably changed the status of the whole document to make it much more substantially about the water question. Also significant were the changes to the account of Watt's family background and circumstances. Initially Arago had made Watt's family origins out to be more humble than they in fact were, perhaps because it fitted his purposes that Watt's rise to eminence be as steep as possible. Thus Arago was concerned 'to show in what a humble condition projects were elaborated, which were destined to raise the British nation to an unheard of height of power'.[66] Watt Jr was anxious to correct this.

Another notable feature of the exchanges is that Watt Jr constantly sought to insert material concerning his father's craft skills and civil engineering work in a way that shows pride in his father's versatility. But Arago resisted changes of this

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63 Watt Jr to Arago, 8 May 1839, Watt Papers (Doldowlod), W/10. 'Mr Baines' book' was Edward Baines, *History of the Cotton Manufacture in Great Britain*, 1835. It is notable that in retailing the story of the Wye estate, Watt Jr was anxious to assimilate the activities and taste of his father and himself. This reinforces again the sense we have of Watt Jr's almost obsessive concern to identify with (and mimic) his father.

64 Watt Jr to Arago, 28 May 1839, Watt Papers (Doldowlod), W/10.

65 Watt Jr to Arago, 31 May 1839, 4 June 1839, Watt Papers (Doldowlod), W/10. See document 'Distribution of Copies of Mr Arago's Eloge. June & July 1839' preserved in Watt Papers (Doldowlod), W/10.

66 Arago, *Historical Eloge of James Watt*, p. 2. It is interesting to compare Cuvier's account of Cavendish in his *éloge* as overcoming the potential obstacle of his wealth in order to do great scientific work. (See Georges Cuvier, 'Henry Cavendish', as reproduced in Eduard Farber (ed.), *Great Chemists*, 1961, pp. 229–38, at p. 229.)

sort. He was much more inclined to see the civil engineering work in particular as having been a waste of Watt's time when he could and should have been pursuing his steam-engine improvements. There are perhaps echoes here of cultural differences concerning the merits or otherwise of specialization.

There were some tensions over Arago's 'steam nationalism', particularly unresolved disagreements about French contributions to the development of the steam engine and the credit due to Papin. We have also seen that Watt Jr had tried to persuade Arago to remove some remarks about Edmund Burke. He failed in this but did have them toned down. In the end Watt Jr was prepared to allow Arago some of his political points. He did not even try to change various sections of the *Eloge* in which Arago pursued matters important to him but tangential to the life of Watt. This was notably true of Arago's long excursus on the social effects of the steam engine and on the question of machinery and the working class.

Apart from these areas of substantive change, the long negotiation between Watt Jr and Arago about the *Eloge* reveals much about their respective characters and perspectives. It confirms our view of Watt Jr as remarkably determined and dogged in pursuit of his filial objectives and willing to go to almost any lengths to achieve them. It also supports our view of him as adopting a strongly empirical perspective on the questions at issue. Arago is revealed as overworked, given to procrastination, but also capable of long bouts of determined work exhibiting rhetorical flourish and strong ideological purpose.

British Versions of the *Eloge*

The translation of the *Eloge* into English had interested Arago from the beginning. In 1834 he hoped that the *Edinburgh New Philosophical Journal* (*ENPJ*) would publish a translation. It did do so, but not until October 1839.[67] The editor of that journal, Robert Jameson, noted the 'considerable delay' that had occurred in printing 'the most important *Eloge* ever written by Arago'.[68] The *ENPJ* had a tradition of publishing *éloges* delivered to the Académie des Sciences, including those of Volta, Young and Fourier by Arago himself.[69] For reasons explained elsewhere,[70] two English versions of the *Eloge* appeared very quickly; the first with various appendices and notes was published by Jameson, and the second with a somewhat different but overlapping suite of appendices and additional notes by Muirhead.

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[67] The *ENPJ* translation (**27**, October 1839, 221–91) was the same text as was published separately as a pamphlet in Edinburgh. The *Journal* also split off and printed 'On Machinery Considered in Relation to the Prosperity of the Working Classes' by M. Arago (297–310). It also printed Brougham's 'Historical Account' (316–24). The final inclusion was 'Additional Notes' on Arago's Memoir by J.P. Muirhead (310–15). This last item, however, was not published in the Edinburgh pamphlet edition of the *Eloge* because of a falling out between Muirhead and Jameson, on which see Chapter 8.

[68] *ENPJ*, **27**, October 1839, 221n.

[69] See Arago, 'Historical Eloge of Alexander Volta', *ENPJ*, **16**, January 1834, 1–33; 'Biographical Memoir of Thomas Young', *ENPJ*, **20**, 1836, 213–40; 'Historical Eloge of J. Fourier', *ENPJ*, **26**, 1839, 1–24, 217–44.

[70] See Chapter 8, below.

These translations of the *Eloge* exhibit at least one major structural difference from the original French publication. One of Arago's 'dissertations', the long section on machinery and the working class that had appeared in the main text of the French version, after the account of the steam engine, was removed from the body of the *Eloge* and appended as a 'Dissertation on Machinery and the Working Class'. Muirhead stated that the change was made in order to give the recounting of the life of Watt greater continuity.[71] There were, however, other reasons. Muirhead and Arago had a different agenda. Arago's political purposes were served by making the machinery question central to the piece. His objectives, after all, had to do with encouraging industrial development in France. These were tangential concerns in the filial project of Watt Jr. Indeed, they were unnecessarily distracting and vexatious in a work designed to commemorate Watt himself. Muirhead provided significant annotations and notes to his translation of Arago's *Eloge*. They confirm the aspects of the *Eloge* that remained of concern to him and to Watt Jr.

In considering the wider reaction to the *Eloge* in Britain in the 1840s and beyond, we inevitably enter into the water controversy itself. The account of Watt was evaluated against Arago's reputation in Britain and in its turn shaped that reputation. So it is important at this point to indicate how Arago was thought of in Britain.

Arago was well known in Britain by the time of the publication of his *Eloge* of Watt. He had visited in 1816 and 1819 on scientific business connected with measurements of the arc of the meridian. He had been awarded the Copley Medal of the Royal Society of London in 1825 and other honours. Arago's researches and his methodology were among those most attractive to key members of the reform group in the Royal Society such as Babbage and Herschel in their quest to raise 'mathematical physics' to a new height in Britain.[72] There was great interest in snaring Arago for the Cambridge meeting of the British Association in 1833, disappointment when he failed to appear, and a proportionate sense of triumph when he did attend the 1834 meeting in Edinburgh. Arago was a key figure in the 'magnetic crusade' of the British Association and a promoter, indeed originator, of that type of 'Humboldtian science'. As Perpetual Secretary of the French Académie, he long remained a vital bridge between British and French science as also in his capacity as editor (with Gay-Lussac) of the *Annales de Chemie et Physique*.

Arago came to be regarded as a larger-than-life character. The brilliant scientist was a stirring and invigorating personal presence who left long-lasting impressions. He was, in short, charismatic.[73] He was also often difficult and disagreeable.

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[71] See 'Preface', Arago, *Historical Eloge of James Watt*, 1839, p. viii.

[72] Maurice Crosland and Crosbie W. Smith, 'The transmission of physics from France to Britain, 1800–1840', *Historical Studies in the Physical Sciences*, **9**, 1978, 1–61; David Philip Miller, 'The revival of the physical sciences in Britain, 1815–1840', *Osiris*, new series, **2**, 1986, 107–34.

[73] John Hope to J.D. Forbes, 20 August 1834, Forbes Papers, Incoming Letters, 1834, no. 26. Hope wanted to offer Arago hospitality during the Edinburgh BAAS meeting because of Arago's kindness to him some twenty years before. An observer of Arago in action in the Chamber of Deputies offered the following description: 'The very moment he enters on his subject he concentrates on himself the eyes and the attention of all. He takes science as it were in his hands: he strips it of its asperities and its technical forms, and he renders it so clear, that the most ignorant are astonished, as they are charmed at

Beneath his dealings with James Watt Jr, which remained friendly throughout, one senses a care on Watt Jr's part to humour Arago and not to push him too far or too hard. Thus, for example, Watt Jr allowed Arago his 'extended dissertations' in the *Eloge*, presumably thinking it unproductive to risk standing up to the great man once too often. When Arago was crossed so decisively by Harcourt, the reports were that Arago was 'very violent' and out to 'pulverise' the unfortunate cleric. Murchison, without reason for exhibiting bravado on Arago's behalf, expected Arago's 'thunder and lightning' to come down on Harcourt's head.[74] Under a thin cloak of anonymity, George Peacock said this of Arago:

> when M. Arago foregoes the high position which the scientific world has assigned to him, and consents, from an unhappy ambition, to put forward views on subjects connected with scientific history which may startle by their novelty or singularity, or gratify a feeling of national vanity … it becomes a public and imperative duty to withstand him.[75]

Arago's stance in the mid-1840s in the controversy over the discovery of Neptune did not win him friends in Britain. All would admit that, by the criteria of priority of discovery that emphasized public communication, Leverrier had published his calculation of the new planet's orbit first. On the basis of Leverrier's predictions, Galle had been the first to 'find' the new planet in the night sky in late September 1846. In this sense Arago's championing of Leverrier was entirely reasonable, especially given Arago's long-standing endorsement of public communication as the real test of discovery. What angered John Couch Adams's supporters was what they saw as Arago's inflexibility and the arrogance and nationalist fervour, with which he pressed Leverrier's, and entirely dismissed Adams's, claims.

After Arago's death in 1853 there was great interest in Britain in his works. Many were translated into English. An evaluation of these works in the *Edinburgh Review* in October 1856 by Baden Powell summarized many of the views of Arago that had circulated in the British scientific community in his lifetime.[76] The impression conveyed there was of a supremely confident young man whose rise had been extremely rapid. Arago had made the most of his opportunities and used colourfully elaborated accounts of his adventures to build mystique. His involvement in politics and, in particular, his 'extreme Republican' views were lamented.[77]

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the ease with which they understand its mysteries. There is something perfectly lucid in his demonstrations. His manner is so expressive that light seems to issue from his eyes, from his lips, from his very fingers … When he is as it were face to face with science, he looks into its very depths, draws forth its inmost secrets, and displays all its wonders; he invests his admiration of it with the most magnificent language, his expressions become more and more ardent, his style more coloured, and his eloquence is equal to the grandeur of his subject.' (See *Edinburgh Review*, October 1856, p. 314.)

[74] Roderick Murchison to William Vernon Harcourt, 28 December 1839, in Jack Morrell and Arnold Thackray (eds), *Gentlemen of Science: Early Correspondence of the British Association for the Advancement of Science*, 1984, pp. 328–29.

[75] [George Peacock], 'Arago and Brougham on Black, Cavendish, Priestley and Watt', *Quarterly Review*, **77**, 1845, 139.

[76] Baden Powell, 'The life and works of Francis Arago', *Edinburgh Review*, **104**, October 1856, 301–37.

[77] Ibid., p. 308.

Although Arago's 'ardent temperament' did have its sweet side, Powell regretfully concluded that Arago's 'moral qualities were not altogether marked by the same elevation as his intellectual faculties':

> In some instances we fear, even those discussions properly belonging to science were not uninfluenced by unworthy and ungenerous passions. His views of the scientific claims of other philosophers, of the priority of discoveries, and similar questions, were too often dictated by prejudice or partiality, party spirit, or national jealousy; while his personal demeanour towards his contemporaries, and especially his subordinates, was frequently offensive from an arrogant, overbearing spirit, displayed both in the affairs of the Academy of Sciences, and the management of the Observatory, as well as in other cases to which his influence extended, so as to obtain for him the sobriquet of the 'Napoleon of Science'.[78]

In this neat fashion common British attitudes to French science and politics were encapsulated in the evaluation of Arago.

Even an admirer like John Herschel was, by the 1850s, very wary of what he regarded as the great Frenchman's slapdash popular writings. Herschel found some of the contents of Arago's popular lectures 'astounding' and hoped that 'for the credit of A's memory' W.H. Smyth and his colleagues, who were engaged in translating Arago, had 'used the pruning hook' more than other translators. Herschel found many things in Arago's publications 'utterly subversive of all rational principle and unworthy of the merest Tyro'.[79] Among a number of the Cambridge group there was a similar tone of exasperation over a talent carelessly, erratically and irascibly applied as one meets in discussions of Arago's coadjutors in the Watt case, Charles Babbage and Henry Brougham. Of course we need to remember that Herschel and others of the Cambridge group were not disinterested parties so far as Arago's reputation and character were concerned, since they had tangled rather strenuously in the dispute over the discovery of Neptune in 1846. In that dispute the Cambridge group had their own political agenda, though it was pursued with a little more reserve than Arago displayed in his histrionics before the Académie. Whilst the Cambridge group were divided on the Watt case, it would be fair to say that attitudes towards Arago hardened after the Neptune controversy. The kind of assessment of him offered by Powell became commonplace after that time.[80] Shortly after Arago's death, Alexander von Humboldt complained at the 'infamous manner' in which the *Quarterly Review* had treated his great friend and attributed the treatment to 'party spirit'.[81]

Smyth et al. heavily annotated their translations of Arago's works, including the *Eloge* of Watt, in order to correct perceived partialities and errors in Arago's

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78 Ibid., p. 311.

79 J.F.W. Herschel to W.H. Smyth, 8 November 1857 and Herschel to R. Grant, 21 March 1858, Royal Society, Herschel Papers.

80 Another dimension of antagonism toward Arago related to Arago's treatment of Thomas Young and his French competitor on the Rosetta stone decipherment. Arago ventured into this in his *éloge* of Young. Smyth et al. comment on this in a footnote in *Biographies of Distinguished Scientific Men*.

81 A. von Humboldt to Varhagen von Ense, 12 December 1853 in *Letters of Alexander von Humboldt, written between the years 1827 and 1858 to Varhagen von Ense*, 1860, p. 217.

accounts.[82] With regard to his biographical writings in particular, Arago was recognized as having great talent. His extensive breadth of knowledge of scientific fields was combined with a 'singularly happy style of lucid eloquence in expounding and illustrating' his subjects' lives and works. This talent was, however, used with partisan intent. British readers would be aware in particular of Arago's nationalistic efforts on behalf of Daguerre and of Leverrier and of his *Eloge* of Watt. Even the latter, although praising Watt and, it was thought, exaggerating his claims against Cavendish for ideological reasons, sought to elevate French achievements in steam-engine development beyond their rightful place.[83]

So by 1857, when the annotated version of the *Eloge* appeared in the translation of Arago's *Biographies of Distinguished Scientific Men*, the reading public were already well prepared to treat it as an untrustworthy and ideologically driven document. Arago's contentious text was effectively enveloped by the explosion of anodyne mid-Victorian biographical writing. Smiles's *Lives of Boulton and Watt* deliberately contrasted itself with the combative style of Arago's and Muirhead's contentious writing.

Conclusion

Arago's *Eloge* of Watt was a complex document in both origin and argument. Arago was certainly driven to undertake it by, and realized through it, a range of ideological objectives as part of his involvement in the politics of French science, technology and industrial development. For these purposes Arago was intent upon assimilating Watt the philosopher and Watt the engineer. However, the *Eloge* was also driven by a nationalism that considered it vital to assert France's role in the development of industrial technology, a role that Arago was arguing should be resumed and extended. Arago's related struggle to open up the Académie to the wider world involved him in seeking to open up its communications. This was linked in a number of his *éloges*, including that of Watt, with the individual's willingness to communicate. Arago used the case of Cavendish as a symbol of non-communication and its consquences.

Arago's decision to seek information and advice from Watt Jr inevitably made him hostage in some degree to the filial project. Watt Jr had some impact upon the characterization of his father in the *Eloge* as an autodidact. He and Brougham between them certainly gave a prominence to the water controversy, and the 'crime

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[82] François Arago, *Biographies of Distinguished Scientific Men, translated by W.H. Smyth, Baden Powell and Robert Grant*, 1857. Smyth et al. state at the point where Arago's *Eloge* deals with the misdating of Cavendish's reprints: 'Our author must have been excited here, for he thinks that not only the high-minded Cavendish and Blagden, but even the printers of the papers, were in a conspiracy against Watt; and, though he calls God to witness that he means nothing against their probity, he makes a very bold insinuation that they were leagued against truth' (p. 573). At another point in the treatment of the water question, drawing a direct contrast with Arago's work, they refer to Harcourt's 1839 'Address' as 'alike free from reckless assertion, and that hot nationality which warps judgment' (p. 575).

[83] Baden Powell, 'The life and works of Francis Arago', 312.

of Cavendish', that it might not otherwise have had. Watt Jr acknowledged, however, that there were limits to his capacity to divert Arago from some of his key ideological objectives in writing the *Eloge*, especially that of associating the forces of conservatism with wrong-headed opposition to technological development. Arago's nationalism, his ready assimilation of science and technology, his attack upon the iconic Cavendish and, it must be said, the association of these actions in the minds of his critics with his ardent republicanism, set up a clash with those who sought to manage the symbols of early Victorian science in Britain.

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Chapter 7

Managing the Symbols of Victorian Science: 'Gentlemen of Science' and the Water Controversy

Introduction

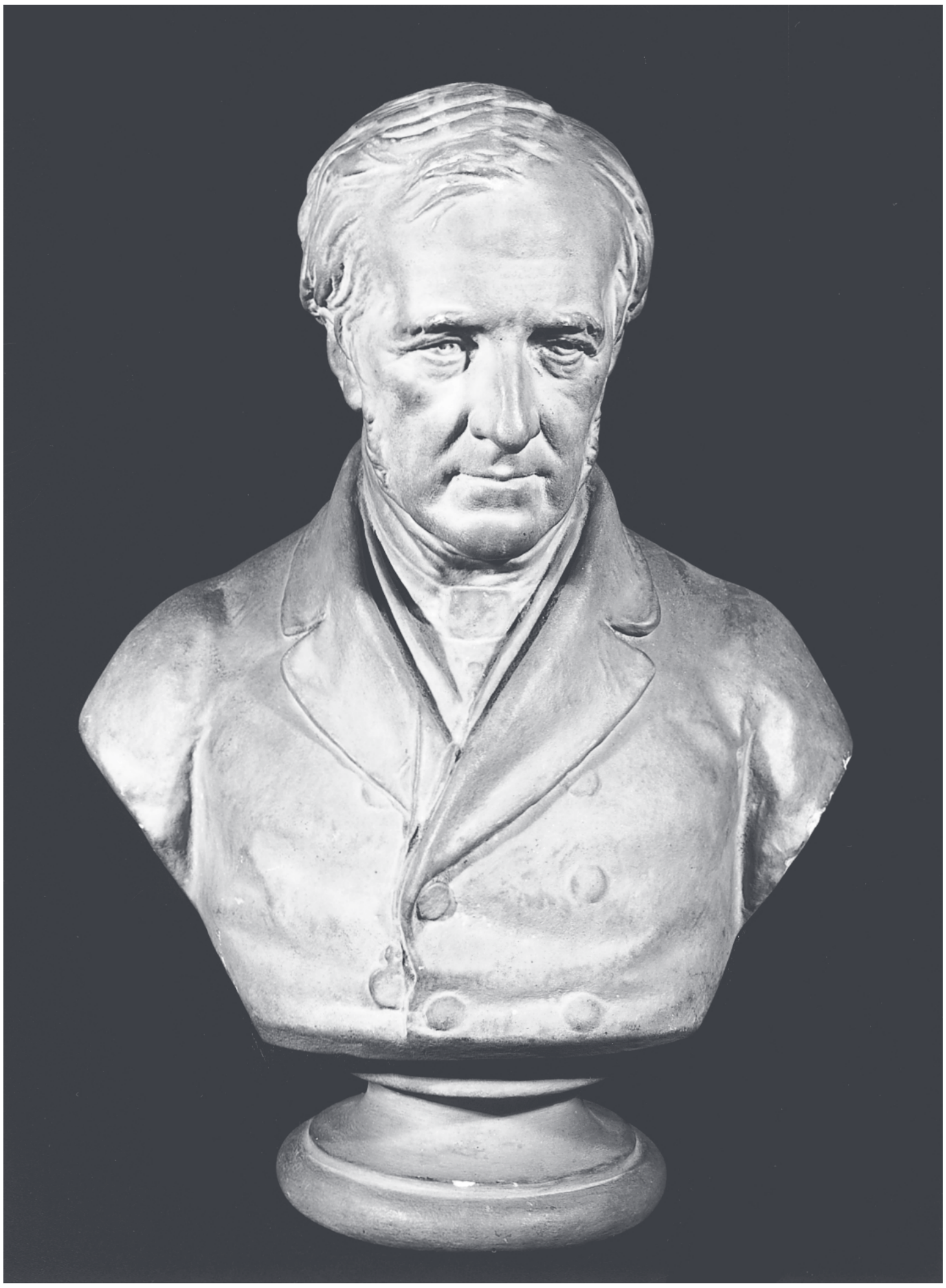
The water controversy was given life in its second phase when Arago and Brougham's challenge was taken up by the Reverend William Vernon Harcourt in his Presidential Address to the Birmingham meeting of the British Association for the Advancement of Science in August 1839. Sir David Brewster tackled the issue also, and in distinctive style. He was to be one of the few major figures to actually change his mind publicly about the water question. In this chapter we examine the substance of Harcourt's 'Address' and also Brewster's writings and conversion, placing them in the context of the British Association's early years and the 'boundary work' engaged in by its leaders. The basic form and internal workings of British science were being negotiated during these years under the growing dominance of metropolitan, Cambridge and Oxford savants. They did not go unchallenged, however. Brewster's struggles with the Cambridge élite in particular intersected substantially with the water controversy and profoundly shaped his, and their, stance within it.

Harcourt Intervenes

William Vernon Harcourt (1789–1871) was the son of the Archbishop of York. He graduated in classics from Christ Church Oxford in 1811, but while at the university, he had developed scientific interests, particularly in geology and chemistry. He pursued a clerical career, was instrumental in founding the Yorkshire Philosophical Society in 1822 and was part of the provincial thrust behind the establishment of the British Association in 1831.[1] By the time we encounter him, Harcourt, in alliance with some members of the Cambridge group, had quickly become one of the chief orchestrators of the Association.

In his extended published response to Arago, which appeared in 1840, Harcourt claimed to show decisively that the credit for the discovery of the compound nature

[1] On Harcourt see *DNB* and Jack Morrell and Arnold Thackray, *Gentlemen of Science: Early Years of the British Association for the Advancement of Science*, 1981, pp. 535–36.



7.1 The Reverend William Vernon Harcourt

of water lay with Henry Cavendish.[2] Harcourt's case in support of Cavendish rested on what he called 'three positions' that are worth quoting at length:

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2 'Address by the Rev. W. Vernon Harcourt', *Report of the Ninth Meeting of the British Association for the Advancement of Science held at Birmingham in August 1839*, 1840, pp. 3–69. Importantly, Harcourt, Whewell and Peacock regarded Harcourt's work as exhibiting sound historical practice compared with the slipshod efforts of Arago and Brougham. For a forceful statement of this position see: [George Peacock], 'Arago and Brougham on Black, Cavendish, Priestley and Watt', *Quarterly Review*, **77**, 1845, 105–39.

(1) The experiments which Cavendish made in the summer of 1781 not only necessarily involved the *notion* (which is the claim set up for Watt), but substantially established the *fact* (which is the claim set up for Priestley) of the composition of water.

(2) The experiment which Priestley made in April 1783, *for the professed purpose of verifying the fact of the conversion of air into water, communicated to him by Cavendish*, added nothing to the proofs which Cavendish had already obtained of it nearly two years before.

(3) Whilst the views of Cavendish are shown by the internal evidence of the experiments themselves, and the train of reasoning which they imply, to have been from the first precise and philosophical, those of Priestley and Watt were always, as regards the former, and till after the publication of Cavendish's and Lavoisier's papers, as regards the latter, vague and wavering to a degree scarcely comprehensible to those who have not studied the ideas prevalent at that period of chemical history.[3]

Thus at least part of Harcourt's case rested upon the claim that Cavendish engaged in a superior, more precise and philosophical approach to a train of research. Priestley and Watt, by comparison, appeared to Harcourt to be 'vague and wavering'.[4] Harcourt acknowledged that the well-versed historian of chemistry might empathize with Priestley and Watt's indecision and confusion given the ideas then prevalent. It was precisely Cavendish's ability to operate within the prevailing theoretical framework without being under its thrall that distinguished him as the superior investigator and hence strengthened his claim to priority.

Elsewhere in his Address Harcourt reinforced these contrasting pictures of Watt and Cavendish as experimentalists and philosophers. Because his ideas about the composition of water were closely bound up with phlogiston theory, Watt was guilty of 'loose reasoning', as were some of the best chemists of the day. Cavendish, on the other hand, steadily moved from 'truth to truth, on every point on which experiments afforded ground for reasoning, unfettered by the complexity of the phlogistic theory'.[5] Cavendish 'alone seemed to understand, as it became a disciple of the school of Newton, the true use of a hypothesis: he valued neither system otherwise than as an expression of facts, or as a guide to future inquiry'.[6] It was not that Cavendish rejected phlogiston theory when he was doing the work on water but rather, Harcourt claimed, that his experiments were effectively independent of theory in the sense that their value was not dependent on particular theoretical presuppositions. Harcourt quoted Humphry Davy on Cavendish's 'finished' technique. Cavendish, according to Davy, 'combined the greatest depth of

[3] 'Address by the Rev. W. Vernon Harcourt', p. 23.

[4] George Peacock, Dean of Ely, and another of the Cambridge clerisy, was to echo Harcourt's case in perhaps even more unflattering terms some years later. Peacock described the theory of the composition of water as proposed by Watt as 'unprofitable and worthless', and Watt's paper in the *Philosophical Transactions* as 'singularly obscure'. According to Peacock, Priestley's experiments, upon which Watt reasoned, were the product of a mind 'not disciplined to the habits of correct inductive reasoning'. Cavendish's memoir, on the other hand, was a model of precision and intelligibility, to be expected from one 'trained in the best and most rigorous school of inductive philosophy'. (See Peacock, 'Arago and Brougham', pp. 114, 131, 133.)

[5] 'Address by the Rev. W. Vernon Harcourt', p. 10.

[6] Ibid.

mathematical knowledge with delicacy and precision' in experimental work. Cavendish's 'processes were all of a finished nature; executed by the hand of a master they required no correction'. Moreover the progress of chemical knowledge had left the 'accuracy' and 'beauty' of Cavendish's work unimpaired. Thus said Davy on Cavendish's death in 1810.[7]

Harcourt argued that because of Cavendish's 'training in the rules of demonstration' and his superior clarity of thought, to him 'hypothetical thoughts and expressions were no stumbling block'.

> If the question then be, who reformed the expressions and logic of chemistry, or who furnished the simple terms in which we now state the elements of water? the answer is, Lavoisier; but if it be, who discovered and unfolded the most important facts on which that reformation relied? who detected and proved the composition of water, and deduced the train of corollaries which flowed from it? the answer is Cavendish. The discovery was not one of those which was within every man's reach, especially in an age of loose experiment and inconclusive reasoning: it was one which could never have been made, but by a strict appreciation of quantities, and a careful elimination of the sources of error …[8]

So, to recapitulate: Harcourt's case for Cavendish was not based just on the contention that he was first to perform certain experiments or to entertain the notion of the compound nature of water (although Harcourt does claim this), but also that Cavendish's claim lay in the nature and quality of his chain of reasoning upon carefully and precisely conducted experiments. Harcourt's definition of the nature and significance of the discovery was such that it closely matched the traits claimed for Cavendish. Thus, Harcourt asked rhetorically what gave importance to the discovery of the composition of water in the history of science? The answer: 'Not merely, as has been too popularly stated, that it banished water from among the elements, but that whilst it accounted for an infinite number of phenomena, it introduced into chemistry distinctness of thought and accuracy of reasoning, and led to the general prevalence of a sounder logic.'[9]

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[7] 'Address by the Rev. W. Vernon Harcourt', p. 7; Humphry Davy, *The Collected Works of Sir Humphry Davy, Bart. LL.D. F.R.S. Edited by his Brother John Davy, M.D. F.R.S.*, 9 vols, 1839, vol. 7, pp. 127–28. Davy's own views on the discovery of the composition of water, as expressed in 1806, and John Davy's commentary thereon, are set out on 130–39. Davy repeated these views in his influential *Elements of Chemical Philosophy*, 1812, p. 37. Watt Jr claimed that Humphry Davy changed his mind on seeing the relevant private correspondence in 1820 and 1826. See Chapter 5 above.

[8] 'Address by the Rev. W. Vernon Harcourt', pp. 12–13.

[9] Ibid., p. 8. George Wilson, the other chief writer for Cavendish, makes a very similar point: 'It is not easy to convey to the general reader, a just conception of the importance which men of science attach to the discovery of the composition of water. It is not merely that a body reputed from the earliest ages an element, has been shown to consist of two altogether dissimilar invisible gases. Hydrogen represents in its properties and relations all the metals and metallic substances in nature: oxygen all the non-metallic ones. Water, which is the union of the two, typifies the constitution of every compound body. All the refined and subtle speculations of the present day concerning the composition of complex substances, are but expansions of the idea which Cavendish's exposition of the nature of water first made familiar to men.' George Wilson, 'Lives of Men of Letters and Science who Flourished in the Time of George III by Henry, Lord Brougham', *British Quarterly Review*, **2**, 1845, 245–46.

Harcourt can be interpreted here as adopting a 'paradigm-dependent' notion of discovery. This may seem odd, because Harcourt was arguing for Cavendish's relative independence from the phlogistic paradigm. But in fact Harcourt was making the attribution of discovery to Cavendish dependent upon Cavendish having prosecuted an exemplary form of disciplined experimental practice. Harcourt emphasized the relationship between credit for discovery and traditions of scientific practice. Harcourt's specific contention was that Cavendish had some claim to have initiated the reformation of chemistry with this work and exhibited exemplary practices. The image of Watt conveyed by Harcourt, by contrast, was of an unsystematic interloper stumbling out of his depth, albeit inventively. In this, Harcourt claimed, Watt had much in common with Priestley, who 'deserves to be admired not more for his inventive fertility and indefatigable industry in experiment, than for the honest candour with which he related every fortuitous success and extraneous hint, and the liberal profusion with which he scattered his gold abroad for public use, as fast as he drew it from the mine'.[10] Priestley exhibited, in other words, a lack of caution, deliberation and discrimination.

It was the approach to scientific investigation, the quantitative skills, the cautious but rigorous reasoning upon experiments which Harcourt praised in Cavendish (and found lacking in Watt) and which he made a criterion for the attribution of discovery. These characteristics were precisely those on which the clerical wing of the Cambridge group among the Gentlemen of Science of the British Association relied in claiming their place as overseers of the scientific enterprise in early Victorian Britain. As previously noted, the conceptions of discovery being rehearsed in the water controversy were part of a larger contest about the nature of discovery, discoverers and originality in innovation. This larger contest incorporated other important controversies in which many of the same groups and individuals were involved. Alignments in the water controversy closely paralleled those in debates about the morality and genius of scientific discoverers, the life and conduct of Sir Isaac Newton, and the patent controversies considering the originality of patentees and whether and how they were to be rewarded.[11]

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[10] 'Address by the Rev. W. Vernon Harcourt', p. 15. The implied contrast here is with Cavendish's famous shyness and unwillingness to publish or part with his work. For the standard Mertonian view of this issue in science communication which, incidentally, relies on the example of Cavendish, see Merton's essay on 'Priorities in scientific discovery', Robert K. Merton, *The Sociology of Science. Theoretical and Empirical Investigations*, 1973, pp. 286–324. On the negotiation of 'communalism' see Michael Mulkay, 'Interpretation and the use of rules: The case of the norms of science', *Transactions of the New York Academy of Sciences*, Series 2, **39**, 1980, 119–23.

[11] Among key writings on these controversies see Richard Yeo, *Defining Science: William Whewell, Natural Knowledge, and Public Debate in Early Victorian Britain*, 1993; idem, 'An Idol of the Marketplace: Baconianism in nineteenth-century Britain, 1830–1917', *History of Science*, **23**, 1985, 251–98; idem, 'Genius, method and morality: Images of Newton in Britain, 1760–1860', *Science in Context*, **2**, 1988, 257–84; Paul Theerman, 'Unaccustomed role: The scientist as historical biographer – Two nineteenth-century portrayals of Newton', *Biography*, **8**, 1985, 145–62. And on the patent controversy and Watt see: Christine MacLeod, 'Concepts of invention and the patent controversy in Britain', in Robert Fox (ed.), *Technological Change. Methods and Themes in the History of Technology*, 1996, pp. 137–53; idem, 'James Watt, heroic invention and the idea of the Industrial Revolution', in Maxine Berg and Kristine Bruland (eds), *Technological Revolutions in Europe. Historical Perspectives*,

Having seen the outlines of Harcourt's substantive published argument concerning priority, we now have to inquire after the circumstances that caused him to enter the fray at all. Can we discover why Harcourt chose to respond to Arago's *Eloge*? We need to backtrack a little to examine, first of all, the proximate circumstances of Harcourt's intervention. The version of Harcourt's Address to the British Association, upon which the above analysis relies, was published in 1840. What was actually delivered at the Birmingham meeting was a much rougher, undeveloped argument. In the interval between the meeting and the publication of the Address, there was significant discussion between Harcourt and his associates about the content and argumentative strategy of the piece.

From newspaper reports and an account in *The Athenaeum*, as well as private remarks of those present, we can tentatively reconstruct the Address as delivered. Harcourt himself stated that it was composed quickly with only limited materials available to him: 'When I wrote my answer to Arago I had only a fortnight to prepare it, & nothing but the publications of Priestley Cavendish & Watt before me.' Muirhead learned from discussions with Professor Edward Forbes, who was present at the occasion, that there was 'hardly any evidence' in Harcourt's speech. It had concentrated on the argument from Cavendish's general character.[12]

As reported by *The Athenaeum*, a report that Harcourt acknowledged as the most accurate one available in the press, the Address certainly began its examination of the water question by concentrating on character. Having set out Arago's claims against Cavendish, Harcourt quoted Davy's panegyric on the great natural philosopher's indifference to fame and his love of truth and knowledge. Then, however, he did make a version of the argument that we see in the published version about the different theoretical universes of Watt and Cavendish, how Watt's ideas were 'infected' by phlogiston theory whilst Cavendish's cleverly remained unfettered by it. Harcourt then elaborated on what he took to be some of the weaknesses of Watt's paper and the strengths of Cavendish's. Thus, whatever one thinks of Harcourt's argument, it is not just from character.

When we turn to local press reports of the speech, *Aris's Birmingham Gazette* gave only a few lines to that part dealing with the water question:

> On this subject [the history of the steam engine] he quoted the history of Watt, as referred to in the memoir by Arago, whose zeal however went too far when he imprinted honours upon him which belonged to the brow of Cavendish. The true merits of the latter were scarcely known to fame; for with all his proficiency in knowledge he was diffident in bringing forward his discoveries, and fearful of the voice of reputation.[13]

The *Gazette* had decided not to burden its readers with any notice of the complex arguments that Harcourt, according to *The Athenaeum*, had indeed offered. The

1998, pp. 96–116; David Philip Miller, '"Puffing Jamie": The commercial and ideological importance of being a "philosopher" in the case of the reputation of James Watt (1736–1819)', *History of Science*, **38**, 2000, 1–24.

[12] Harcourt to Forbes, 11 January 1840, Forbes Correspondence, Incoming 1840, Item 2; J.P. Muirhead to James Watt, 14 October 1839, Muirhead Papers, MS GEN 1354/365.

[13] *Aris's Gazette*, 2 September 1839.

Birmingham Journal, the other major local newspaper of the time, reported rather differently. Although it too concentrated upon Harcourt's remarks about Cavendish's character, it then acknowledged that Harcourt had delivered an 'elaborate dissertation' and reported his comparison of Cavendish's judicious use of hypothesis with that made by others. We are then told that 'this part of the address, which occupied some time, was received with marked applause'.[14] All in all it appears that Harcourt's Address as delivered to the General Meeting was not limited to arguments from Cavendish's character.

When Muirhead said that it *was* so limited, and that there was 'hardly any evidence' in it, he was referring to the fact that Harcourt did not provide *circumstantial* evidence to contradict Arago (and Brougham) on the question of the priority of publication or the accusation of fraud.[15] For Muirhead and other members of the Watt camp, who assumed that Watt's and Cavendish's discoveries were essentially the same, such circumstantial evidence was the only kind of evidence that mattered. Thus Muirhead regarded Harcourt's evidence for the chemical and methodological differences between the two protagonists as no evidence at all.

In the aftermath of the meeting, the correspondence back and forth between Harcourt's friends in the British Association evinced a general acknowledgement that Harcourt had much work to do in developing a published version of his response to Arago's *Eloge*. The Wattites' discussions of Harcourt's spoken address indicated that they considered that he had shot from the hip without a defensible case and that some of the wiser of Harcourt's friends were realizing that he had got himself in a scrape. Watt Jr had learned from some of those present that the address 'did not gain applause, nor give satisfaction'. (The *Birmingham Journal*, on the contrary, reported regular, bracketed, 'Cheers' in its account, though it also, paradoxically, recorded that the meeting offered 'no accommodation for reporters'!) Watt Jr also understood that 'but for the decorum due to his [Harcourt's] rank & station there, some of the parties present would actually have hissed'.[16] If Harcourt persisted, he would be either readily 'skewered' by Watt Jr, or broken by Brougham like a 'butterfly on the wheel'.[17] Failing this, or perhaps in addition for good measure, 'the unfortunate aristocratic Canon of York, the worthy founder of the British Association, will be pulverized' by Arago.[18]

The stir created by Harcourt's Address among those he called the 'Wattites' was reported by the geologist Roderick Murchison:

> The bold shot which you fired at Birmingham in re 'Cavendish versus Watt' was, as you might indeed have anticipated, sure to bring down Arago's thunder and lightning on your head. In the meantime Watt himself [that is, James Watt Jr] is much *up*, and with all his

[14] *Birmingham Journal*, 31 August 1839.

[15] This is confirmed by Muirhead's response to Harcourt in a note in his translation of Arago's *Eloge*. See François Arago, *Historical Eloge of James Watt … Translated … by James Patrick Muirhead*, 1839, pp. 114–16, note.

[16] Watt Jr to Muirhead, 28 September 1839, Muirhead Papers, MS GEN 1374/333.

[17] Muirhead to Watt Jr, 10 and 12 September 1839, Muirhead Papers, MS GEN 1374/310 and 311.

[18] Watt Jr to Muirhead, 22 October 1839, Muirhead Papers, MS GEN 1374/379. The words are J.B. Pentland's, quoting his friend Arago, and quoted to Muirhead in this letter by Watt Jr.

> friends, or rather they for him, have prepared a refutation of what they consider your misprision of the great steamer.[19]

The refutation in question was Muirhead's translation of Arago's *Eloge* with its notes and annotations.[20] Murchison, having learned that Arago and Brougham were also at work on a response, had consulted the naturalist Robert Brown. Brown had been librarian to Banks and close in other respects to Cavendish, and he was certain that Arago was wrong and that Harcourt was 'essentially correct'. Murchison noted that others were becoming involved: 'Just before I left town new actors appeared on the stage. Watt [Junior] is you must know a great friend of Chantrey, and I found from the latter that all the Wattites (Babbage included) had taken up your discourse as a downright *attack* upon their hero.' Murchison reported further that:

> Chantrey's house was to be the scene of a Watt conclave on the day of which I speak and Babbage was one of the 'priés'. What transpired at the dinner of course I know not. You are perhaps aware that Babbage and Arago run in the same curricle in science, politics and their views of human nature.[21]

This last observation was a revealing one. Watt had come to symbolize the industrial temper and became a hero to those who most wholeheartedly embraced industrial progress. We have seen already the ideological work that Arago sought to do with his account of Watt. Brougham, too, actively deployed Watt in arguing for the necessity and viability of the education of working men, in whose ranks he rather artfully included the great engineer. Babbage, and also John Herschel, from their young Cambridge days pursued an industrial philosophy of mind. We know of Babbage's admiration of Watt and that he regularly sent his works (such as *On the Economy of Machinery and Manufactures*) to Watt Jr.[22] Ashworth suggests that Babbage and Herschel saw themselves as the 'philosophical equivalents of great industrialists such as James Watt, Matthew Boulton, and William Strutt'.[23] In this they had many affinities with Arago and ever fewer with their former Cambridge allies among the Gentlemen of Science. We know that Babbage's admiration for Watt, and what he was taken to represent, continued and it is no surprise to find him among the 'Wattites' in the aftermath of Harcourt's Birmingham Address. Babbage had long fought for the Birmingham meeting of the BAAS as recognition of the

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19 Roderick Murchison to William Harcourt, 28 December 1839 in Jack Morrell and Arnold Thackray (eds), *Gentlemen of Science. Early Correspondence of the British Association for the Advancement of Science*, 1984, pp. 328–29. The following draws on Miller, '"Puffing Jamie"', 14–17. On Robert Brown's activities regarding the water controversy see Chapter 10, below.

20 Arago, *Historical Eloge of James Watt*.

21 Murchison to Harcourt, 28 December 1839, in Morrell and Thackray (eds), *Gentlemen of Science. Early Correspondence*, p. 329.

22 See the acknowledgments of receipt of these works in Watt Jr to Babbage, 25 February 1828 and 29 July 1832. British Library, Add. MSS 37184, f. 110 and 37187, f. 54. James Watt Jr also signed *The Times* declaration in favour of Herschel for the Presidency of the Royal Society in 1830 at Babbage's instigation. See Watt Jr to Babbage, 29 November 1830, British Library Add. MSS 37,185, f. 360.

23 William J. Ashworth, 'Memory, efficiency, and symbolic analysis: Charles Babbage, John Herschel, and the industrial mind', *Isis*, **87**, 1996, 629.

importance of industrial Britain to scientific endeavour. He had in fact resigned from the Council of the organization in late 1838 after a row with Murchison about the appointments of President and Vice-President for the Birmingham meeting. Watt Jr also had concerns about the Association's antics in Birmingham. He considered its public display of achievements in the industrial arts to be 'suicidal folly' so far as protecting Britain's industrial interests was concerned. Given all this, to have Harcourt devalue the achievements of Watt, the home-town hero, was just too much for Babbage and the 'Wattites' to bear. For others, though, Watt had come to symbolize the worship of industrial mechanism at the expense of all else. William Whewell was among those in the clerical wing of the Cambridge group who regarded this as dangerous for society and for science.[24] It was from this quarter that the strongest support for Harcourt came.

The 'Gentlemen of Science' and the Cultural Politics of Science

The 1839 arguments over Watt were skirmishes in an ongoing battle. To understand its roots we need to be aware of some of the deeper background of the cultural politics of science in early nineteenth-century Britain. Those years were tempestuous and eventful ones for British science. They had seen attempts at wholesale, and considerable *de facto*, reform of the Royal Society of London. The policies of Banks's Presidency to preserve what I have called a 'Banksian Learned Empire' had continued to give men of learning from across a broad spectrum of the upper ranks a significant role in the Society. This intellectually inclusive, but socially exclusive, strategy came under attack from scientific devotees who sought greater control over their own affairs and reforms that would render the Society more clearly and exclusively scientific. This process was well under way long before the 'official' reforms of the late 1840s.[25] The same reformist groupings engaged in institutional innovation themselves of both a specialist variety (the Astronomical, Geological and, later, Chemical Societies, for example) and in the entirely novel form of the British Association. The vigour of new provincial scientific organizations in the late eighteenth century had continued into the nineteenth century. Significant regional and local groups of practitioners of science emerged and even larger groups of followers of science provided a willing and supportive audience for

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[24] On the troubles surrounding the Birmingham meeting see Morrell and Thackray, *Gentlemen of Science: Early Years*, pp. 251–52, 264 and Morrell and Thackray (eds), *Gentlemen of Science. Early Correspondence*, pp. 276–88.

[25] For the 'Banksian Learned Empire' and the reactive reform movement see David Philip Miller, 'The Royal Society of London 1800–1835: A study in the cultural politics of scientific organization', unpublished PhD Dissertation, University of Pennsylvania, 1981. On the reforms of the 1840s see: Roy MacLeod, 'Whigs and Savants: Reflections on the reform movement in the Royal Society, 1830–1848', in Ian Inkster and Jack Morrell (eds), *Metropolis and Province. Science in British Culture 1780–1850*, 1983, pp. 55–90; Iwan Rhys Morus, 'The politics of power: Reform and regulation in the work of William Robert Grove', unpublished PhD thesis, University of Cambridge; Iwan Rhys Morus, 'Correlation and control: William Robert Grove and the construction of a new philosophy of scientific reform', *Studies in History and Philosophy of Science*, **22**, 1991, 589–621.

formal educational lectures, rational entertainments, local scientific meetings, and for the visits of the travelling scientific circus that the British Association became. Greater reliance upon technology and scientific approaches in industry and commerce, as well as in agriculture, facilitated the growth of various cadres of scientific and technical specialists, while educational institutions and new publications catered to their needs also. The 'scientific community' rapidly became more diverse and differentiated. Looked at from the perspective of emergent scientific élites, there was much vitality but a problem of control within a burgeoning system. The British Association became a particularly important vehicle for the exercise of such control. As Morrell and Thackray[26] amply demonstrated, there were divergent plans for the new Association in the beginning, but quite quickly it came to be dominated by an élite group of 'Gentlemen of Science'. While working hard to promote scientific research and a positive evaluation of science in society, this group was also engaged in an effort to control burgeoning scientific activity. They sought to define science's relations with other institutions (its boundaries) in a way that promoted their own version of the scientific future and their own scientific interests.

Although its inadequacies are apparent, the term 'Cambridge Network', coined by Susan Cannon to describe a particular grouping within British science at this time, is a useful place to start in depicting the Gentlemen of Science. Cannon used the term to refer to a constellation of individuals variously associated with Cambridge University and devoted to science, who exhibited a range of characteristics such as broad church sensibilities and an interest in scientific and educational reform. An early node of this network was the 'Analytical Society' in which the young Charles Babbage, John Herschel and George Peacock played a leading part. Their programme of modernization of the teaching and use of mathematics at the university, and more widely, subsequently attracted people like William Whewell and George Biddell Airy.[27]

The careers of the members of the Cambridge Network were diverse, however. Whewell and Peacock took religious orders and built their careers at the university, holding a range of professorships and college positions. Whewell rose, in 1841, on Robert Peel's nomination, to the Mastership of Trinity College, rightly considered as one of the most powerful positions in the country.[28] Peacock, under different

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[26] Morrell and Thackray, *Gentlemen of Science: Early Years*.

[27] On the Cambridge Network see Walter Faye Cannon, 'Scientists and Broad Churchmen: An early Victorian Intellectual Network', *Journal of British Studies*, **4**, 1964, 65–88 and an elaborated version of that paper in Susan Faye Cannon, *Science in Culture: The Early Victorian Period*, 1978, pp. 29–71. On the Analytical Society see Philip C. Enros, 'The Analytical Society (1812–1813): Precursor of the renewal of Cambridge mathematics', *Historia Mathematica*, **10**, 1983, 24–47. Other important insights into this grouping are provided by the writings of William J. Ashworth and Timothy Alborn.

[28] On Whewell see Richard Yeo, *Defining Science*, 1993 and Menachem Fisch and Simon Schaffer (eds), *William Whewell: A Composite Portrait*, 1991. Thinking of the sources of Whewell's power, I noted in the Whewell Correspondence that a number of individuals who were involved in the water controversy in some way corresponded with Whewell about their sons' admission and attendance at Trinity or another Cambridge College. See, for example, Harcourt to Whewell, 29 January [1846], Whewell Papers, Trinity College Cambridge, Add. Ms.a.205$^{136(1–2)}$ regarding his second son going to Trinity; Macvey Napier to Whewell, 19 January [1836?], Add. Ms.a.210^{11} regarding his son Alexander

political circumstances, might have sought and obtained that position, but the Deanery of Ely offered some compensation. George Airy held successively the Lucasian and Plumian Professorships at Cambridge until his departure for Greenwich Observatory and the Astronomer Royalship in 1835. He held that position for almost fifty years and transformed it into the apex of one of the great scientific bureaucracies of the nineteenth century.[29] Babbage and Herschel took rather different trajectories, thanks largely to their independent financial means. Although at various times they sought, or were importuned to take, university professorships, they ended up retaining the gentlemanly independence to be active in metropolitan scientific institutions and to pursue their trains of research. Babbage was happier venting his misery and frustration on official bodies than in serving them.[30] It is perhaps significant that those members of the Cambridge Network who retained strong career connections with Cambridge University (that is, Whewell, Peacock and Airy) were the most conspicuous supporters of Cavendish in the water controversy. Babbage, though not making it one of his major causes, does appear to have sided with the Watt camp. Herschel, characteristically reserved and anxious to avoid controversy, made noises sympathetic to both Cavendish and Watt. He was certainly more prepared than the Trinity Men were to grant Watt some degree of credit in the affair.[31]

The limitations of the term ‘Cambridge Network’ appear not just because, over time, there were splits of various sorts between its putative members, but also

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who was studying divinity at Trinity; Earl of Burlington to Whewell, 5 and 9 November 1850, Add. Ms.c.88[23–24] on looking after his son Spencer Compton, Lord Cavendish.

[29] On Airy see Robert W. Smith, ‘A national observatory transformed: Greenwich in the 19th century’, *Journal for the History of Astronomy*, **45**, 1991, 5–20 and Allan Chapman, ‘Science and the public good: George Biddell Airy (1801–92) and the concept of a scientific civil servant’, in N.A. Rupke (ed.), *Science, Politics and the Public Good: Essays in honour of Margaret Gowing*, 1988, pp. 36–62. On Peacock see M.J. Durand, ‘Le Travail mathématique de George Peacock (1791–1858)’, *Sciences et Techniques en Perspective*, **11**, 1986–87, 91–151.

[30] On Herschel and Babbage see Gunther Buttmann, *The Shadow of the Telescope. A Biography of John Herschel*, 1970; H.W. Buxton, *Memoir of the Life and Labours of the Late Charles Babbage*, ed. Anthony Hyman, 1988; Simon Schaffer, ‘Babbage’s Intelligence: Calculating Engines and the Factory System’, *Critical Inquiry*, **21**, 1994, 203–27; William J. Ashworth, ‘Memory, efficiency and symbolic analysis: Charles Babbage, John Herschel, and the industrial mind’, *Isis*, **87**, 1996, 629–53.

[31] Muirhead to Brougham, 18 February 1848, Brougham Papers, 23,380 reported that Herschel, in the wake of Jeffrey’s article, ‘while he gives Lavoisier the greatest credit, says that Watt is clearly titled to the credit of the first subjective discov[y] and that Cavendish made the first objective discov[y]’. Earlier, in thanking Muirhead for a complimentary copy of the *Correspondence*, Herschel had written a long letter explaining his understanding of the controversy. There he confessed that it was hard to judge the controversy because ‘I had been so completely accustomed (with the generality of readers) to look upon Cavendish as the discoverer of the composition of water’. Herschel also noted ‘the almost impossibility of placing one’s self back in time & knowledge, and bringing one’s ideas into that state of confusion in which it must have presented itself to the other Chemists of that day’. After a long disquisition, Herschel divided the ‘discovery of the fact’ between Cavendish (the Lion’s share), Priestley, Warltire and Macquer, while he gave the interpretation to Lavoisier, ‘Watt not being however entirely excluded’. (Herschel to Muirhead, 12 March 1847, copy of draft, Herschel Papers, Royal Society of London, vol. 22, 47.3.12.1–6. It is not clear that this letter was ever sent to Muirhead.)

because the political formations of the élite of British science soon transcended the Cambridge group. To be sure, some members of that élite were students of the Cambridge men, or admirers of them, James David Forbes in Edinburgh being a prominent example. Others, however, came from analogous positions at Oxford University (Baden Powell, William Buckland), or from the ranks of metropolitan savants, like the mathematical practitioner, insurance expert and astronomical dogsbody Francis Baily.

The British Association became a key forum in which the 'Gentlemen of Science' worked out their role within the burgeoning scientific enterprise in Britain. There were significant differences of opinion about what the role of the Association should be. The Association began as a provincial initiative and the more radical reformers, like Babbage and Brewster, had very definite ideas about what it should do, envisaging in particular a very active lobbying of government for financial support of science and its practitioners. Initiatives such as the reform of the patent laws were seen by Babbage and Brewster as a suitable cause for the Association to pursue. Babbage was very keen to involve the industrial areas of the country centrally in the Association's affairs and meetings.

These early plans were moderated and modified by an alliance of Oxbridge clerics and some of the original provincial founders, notably Harcourt and John Phillips. In the 1830s, under this control, the Association did lobby government, but primarily for assistance with international collaborative scientific projects. There were some serious reservations about involving government directly or substantially in scientific activity. The gentlemanly voluntarists saw little need for the government to provide employment for devotees of science. There was, after all, great pride in the British voluntarist tradition and what it had achieved, together with considerable suspicion of the centralized French system that intruded the distractions and distortions of party politics into many scientific lives. By the 1840s and 1850s, the career of Arago had for many become an object lesson in the dangers of such patterns of institutional development. Others, however, including Babbage and Brewster, held up the French system as a model of best practice in many respects and lamented the inadequacies of British organizations.

Those who gained control of the Association sought to support the research of individuals, but they did so from the funds of the Association itself. By gathering subscriptions and running meetings at a profit, the Association generated funds that were disbursed to worthy projects. In this way supporters of science around the country who attended the Association's annual meetings were harnessed to finance the research of the scientific élite of the organization. Money was allocated to sections of the Association and drawn to cover expenses incurred by those charged with projects selected for support. The whole process was overseen by a Committee of Recommendations dominated by the 'Gentlemen of Science'. Between 1833 and 1844, according to Morrell and Thackray, well over £20,000 was allocated to the sections in total for support of research. Of this, almost 60 per cent went to Section A (Mathematical and Physical Sciences). Other well-supported sections were Section C (Geology) and Section G (Mechanical Science), both of which received over £3000, or more than 10 per cent of the total allocation. These amounts were, however, not all spent. Over that period only about £12,000 was drawn. Of that sum, almost half went to the top seven recipients and 90 per cent of the money

spent was expended by Sections A, C and G.[32] Some members of the inner circle were beneficiaries of these expenditures: Baily headed the list of grantees, Whewell was third and Harcourt was in the top ten. But generally the projects were defensible, and defended, as involving large-scale data gathering and dealing with topics of great communal interest and importance. As Morrell and Thackray put it, the structure of governance of this research support was 'another device by which the inner cabinet of the Association controlled the General Committee on such important matters as nurturing chosen careers, reinforcing approved ideologies of science, and promoting preferred sorts of scientific work'.[33]

Medical areas, social science and statistics fared poorly in obtaining research support. Care was taken to circumscribe such troublesome areas and they received limited attention. Those seeking the kind of scientific affirmation that it could give continually troubled the Association. The phrenologists dogged the Association's steps for many years on an imitative peripatetic route but were kept at bay. However, the boundary-management that concerns us most centrally in our story is that relating to science and technology or 'Art'.

The Association's section on Mechanical Sciences was established in 1836 as Section G. In line with the attitudes towards the relations of science and the practical arts evinced by Whewell, the constitution and conduct of Section G affirmed the ultimate superiority of the mathematical and physical sciences to engineering practice. The role of the Association was as a scientific arbiter supposedly above the rough and tumble of the invention business. This stance emerged in part from the Association's decision not to follow David Brewster's early suggestion that the Association lobby for patent law reform. What Morrell and Thackray describe as the 'consolidation' phase of the Association's early history in which it visited Oxford, Cambridge, Edinburgh and Dublin put the mechanical arts at a low priority. Various planned initiatives foundered in these early years, including one for Watt Jr to prepare a report on the law of resistance of water to bodies in motion, especially steam-driven vessels.[34] In the mid-1830s, however, a group interested in engineering affairs emerged and the Association's controlling group were ready to deal with issues of the relationship of science to technological progress:

> The assumption of the Association's apologists was that theory and practice were sharply distinct yet connected in certain significant ways. It was true that the mechanical arts often marched independently of mechanical science. But the best route to true, enduring, and satisfactory practice depended on theory … Theory that was encoded and manipulated via abstract symbols took precedence. Mechanical *science*, transmissible by symbolic language, and not the mechanical *arts*, transmitted by personal demonstration, was to be increasingly professed within the Association.[35]

The banner of 'mechanical *science*' could attract the interest of various groupings. Manufacturers found scientific ratification of their activities; engineers detected a

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[32] Morrell and Thackray, *Gentlemen of Science: Early Years*, p. 317.

[33] Ibid., pp. 309–10.

[34] Ibid., p. 258. (The suggestion regarding Watt Jr is in Harcourt to Whewell, 20 July 1832, Whewell Papers, WP.a.205[126].)

[35] Morrell and Thackray, *Gentlemen of Science: Early Years*, pp. 259–60.

basis for their claimed higher status 'as mediators between the natural philosopher and the working mechanic'.[36] Morrell and Thackray show that Section G operated with personnel and structures that clearly placed practical men (engineers) under the control and effective supervision of the university teachers who commanded mechanical science. Officers of the Institution of Civil Engineers such as James Walker and, especially, Thomas Webster were involved with Section G. Walker, who was President of the Institution between 1837 and 1845, was a Vice-President of Section G in 1840. Webster, who was employed by the Institution, served as the Section's Secretary for a number of years. Academic engineers or people with strong scientific backgrounds were more conspicuous in the President's chair and among the Vice-Presidents of the Section.

The engineers were by no means totally subordinated. Sometimes they criticized the work of Section G as impractical. Walker's Presidential speech to the Institution of Civil Engineers in 1841 evinced concerns about the development of academic engineering and the production of too many theoretically qualified engineers whose knowledge exceeded, and levels of practical skill fell short of, the requirements of the job market.[37] Nevertheless the perceived value of cooperation with the Association must have outweighed such concerns. The engineers' position within the Association strengthened in the late 1840s and beyond, though it was not until 1861 that William Fairbairn became the first engineer to assume the Presidency.[38]

The élite dominated engineering within the Association by virtue of their superior command of mathematics and physical science. A number of Cambridge men were so involved. Charles Babbage's engineering interests are well known. William Whewell produced a well-timed text in 1841, *The Mechanics of Engineering. Intended for Use in Universities, and in Colleges of Engineers*. Whewell explained that the work was intended not only to help the education of professional engineers but also to contribute to a liberal education:

> If the common Problems of Engineering were to form part of our general teaching in Mechanics, this science also might become a permanent possession of liberally educated minds. Every roof, frame, bridge, oblique arch, machine, steam-engine, locomotive carriage, might be looked upon as a case to which every well-educated man ought to be able to apply definite and certain principles in order to judge of its structure and working.[39]

The tenor was clearly that the custodians of mechanics had a deal to teach both engineers and the liberally educated gentleman. Moreover the latter, when in command of the principles of mechanics, would be in a position to judge the work of the former.

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[36] Ibid., pp. 260–61.

[37] See James Walker in *Proceedings of Institution of Civil Engineers*, 1841, 25–26, as quoted and discussed in R.A. Buchanan, *The Engineers. A History of the Engineering Profession in Britain, 1750–1914*, 1989, pp. 164–65.

[38] Crosbie Smith, *The Science of Energy. A Cultural History of Energy Physics in Victorian Britain*, 1998, pp. 140–49 makes the important point that the leadership of the Association was in fact changing with a new generation of more entrepreneurial energy physicists and engineers.

[39] William Whewell, *The Mechanics of Engineering. Intended for Use in Universities, and in Colleges of Engineers*, 1841, p. v.

George Airy was notable for a deep and lifelong interest and involvement in engineering problems. As Lucasian, and then Plumian, Professor at Cambridge in the late 1820s and early 1830s, Airy lectured about, among other things, bridges, trusses, toothed wheels, wedges, screws, pulleys, the theory of roofs, arches, domes and groins. During his long tenure as Astronomer Royal, Airy was well known for his ingenuity in dealing with engineering problems. He was frequently consulted by engineers, including Stephenson and Brunel, and helped particularly with the Britannia Bridge over the Menai Straits.[40] Elected an Honorary Member of the Institution of Civil Engineers in 1842, Airy agreed to serve on its Premium Committee, helping to set prize questions and assess responses to them. Some of Airy's suggested topics for premiums capture well his, and his associates', ideas about scientific engineering, as well as their conception of the hierarchical relationship between themselves, the scientifically informed engineer and the practical engineer.[41]

Returning now to the British Association and the management of the boundary between science and practice, we can see more clearly the attitudes encapsulated in the superior, controlling stance of the leadership over matters of engineering practice. These attitudes lay behind relations between the Cavendish and Watt camps. We know that an invitation to Watt Jr to report to the Association was contemplated in 1832 and that the suggestion was to be that he report on the law of the resistance of water to bodies in motion with particular reference to steam-driven vessels. It is not clear whether an invitation was ever made or, if made, what the response was. It is hard to avoid the feeling, however, that if the invitation had been made it would probably have brought into stark relief Watt Jr's lack of competence as a scientific

[40] See T.J.N. Hilken, *Engineering at Cambridge University 1783–1965*, 1967, pp. 45–50.

[41] See RGO 6/401, ff. 246 and following for correspondence regarding his election and premiums. Among these papers we find one of Airy's suggestions for a premium topic that illustrates his approach well. It was one on steam engines. First he explained the background to the topic: 'I have examined probably hundreds of steam engines, in a cursory way, while they were at work, and have usually found some of the standing adjustments upon which the engineers know nothing or next to nothing. One of these is the state of the condensometer. Not one engine in ten has [this] in order, and not one engineer in ten has any notion of judging of the state of the condensation from the temperature of the discharged water. It is well known that power is lost by condensing too completely, though I believe no rule, theoretical or practical, exists, by which the proper degree of condensation can be ascertained. And even when that is ascertained, as most advantageous for the mechanical power, it may not be free from inconvenience as regards other considerations, thus the only distinct reason that I ever got from an engineer for not condensing more closely was "that would make the engine jump". I would therefore submit whether the following subject is worthy of being proposed for premium: "To ascertain, from theory or from direct experiment, the degree of condensation which is most favourable for the working of a steam engine, as regards the production of mechanical power: to ascertain also whether any inconvenience is actually produced in any other respect by this degree of condensation, and to show how such inconvenience may be removed: and to give simple rules for the temperature of the discharged water or other indication, adapted to the use of engineers, for securing the proper degree of condensation".' This question captures well the assumed relation between the scientific elite (Airy in this case), the scientifically competent engineer and the merely practical engineer. The first could set the question and judge the quality of answers to it, the second could develop solutions to it, the third could, with suitably simplified instructions, implement the fruits of the investigation without understanding it.

engineer as judged by the standards of the early 1830s. Even if Watt Jr had been competent to so report, it is likely that, given his attitude to the Association and its fundamental violation of proprietary interest, and the danger that it posed, as he saw it, to private individual and national trade, he would still have refused to become involved.

One further example captures very well how the issue of the relations between science and practice was linked to the water controversy. This concerns the Association's work on the hot-blast technique of iron production. The interest of key members of the Watt camp with the defence of James Beaumont Neilson's patent on this technique will be discussed further in the chapter on the advocates of Watt. Suffice it now to say that Lord Brougham had drawn up Neilson's patent specification in 1828 and that Muirhead was closely interested in the defence of that patent. Parallels were drawn explicitly at the time between the difficulties that Neilson had and those experienced by James Watt in dealing with people who sought to circumvent their patents. The two men were also compared in terms of their scientific approach to their inventions.

It will be remembered that the Association had avoided direct involvement in the debate on patent law reform and so stood aloof from the kind of battle involved in relation to Neilson's patent on the hot-blast technique. The Association, however, did involve itself with the study of the hot blast in a characteristic fashion. Its stance epitomized the expert arbitrating role that the Association sought to assume in seemingly rising above matters of technological dispute and operating in the 'public' interest.[42]

The initiative to commission a report on the hot-blast technique appears to have originated with Harcourt. Harcourt was a close associate of Earl Fitzwilliam, who had direct iron-smelting interests. At the Dublin meeting of the Association in 1835 the manager of Fitzwilliam's Elsecar works, Henry Hartop, questioned the quality of iron produced using the hot-blast technique. The focus of the Association's inquiries into the technique concerned its efficacy when compared with the traditional cold-blast method. Eaton Hodgkinson and William Fairbairn conducted the inquiry and concluded from an elaborate series of experiments and measurements that, although there was considerable variation in quality in various respects, the energy savings of the hot-blast technique favoured its general adoption.[43] This allowed the maintenance of some distance from the patent disputes then raging. But the Association's intervention would not have pleased the Neilson camp because, despite its overall endorsement of the technique, it documented its lack of uniform efficacy. This could have been taken by Neilson's opponents as ammunition to argue the incompleteness of the specification. Whether it was so used or not does not matter. Neilson's supporters, like Brougham and Muirhead and Watt Jr, would, I suggest, have perceived the intervention by the Association in that way. To them

42 To an extent, then, the gentlemanly ethos of the Association carried on earlier thinking about the 'practical' and the 'philosophical', practical projects being regarded as disinterested and philosophical in so far as they were pursued in the public, rather than particular private, interest. See David Philip Miller, 'The usefulness of natural philosophy', *The British Journal for the History of Science*, **32**, 1997, 185–201.

43 Morrell and Thackray, *Gentlemen of Science: Early Years*, 497–98.

the scientists would appear to be blundering in on the topic professing 'neutrality' and scientific indifference to proprietary interests and yet, in fact, giving potential comfort to one side of the struggle. The existence of tension between the Watt camp and the self-appointed scientific arbitrators of the Association is not surprising.

The visibility of practical engineering achievement was a fact of life in early Victorian Britain that the leadership of the Association had to deal with. Their concern was to establish a relationship between science and practice that linked science to those achievements but maintained a careful and significant differentiation between them. Mechanical *science* provided the link. The élite's custody of the mathematically and experimentally based research, upon which, they argued, practice must increasingly rely, provided the differentiation.

There were a number of occasions when the ideology of the relations of science and practice was elaborated at length. An early occasion was at the Association's Cambridge meeting in 1833. William Whewell gave an address in which he argued that a clear distinction must be retained between theory and practice. As Richard Yeo has put it, Whewell 'wanted science to be lauded justly for its intellectual value, rather than incorrectly for its useful application'.[44] He described 'Art' (that is, practical industrial application) as the 'comely and busy mother of a daughter of a far loftier and serener beauty', that serener beauty being, of course, the contemplation of scientific truth.[45] Whewell's *History of the Inductive Sciences* (1837) gave only brief and grudging attention to the practical applications of science.

Whewell's general stance on this was relevant to his interventions in the water question. It is no coincidence that Whewell and George Peacock were very supportive of Harcourt's crossing of swords with Arago. The Watt camp seems to have considered that Harcourt had not acted alone in this. Arago, via his conduit Pentland, vented his spleen over Harcourt's attack, noting that Watt's name 'will be green in the memory of the world, when that of the Harcourts, the Whewells, & the Philips his advisers & abettors, will be for ever buried in oblivion'.[46] Muirhead reported to Watt Jr that Harcourt was 'the tool of others, and that he would not have ventured on such an "adventure perilous" on his own responsibility'.[47] There is other evidence supporting the existence of a pro-Cavendish coterie, though prior involvement of others in the decision for Harcourt to speak on the question cannot be proven. Harcourt confided to his wife the day after the Address, that he got through his 'principal work last night very successfully … and putting aside common compliments, I am glad to find that the Dean of Ely [George Peacock], of whose judgment I have a great opinion, is highly satisfied with me'.[48] William Whewell

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44 *Report of the Third Meeting of the British Association for the Advancement of Science held at Cambridge in 1833*, 1834, xxiv–xxv; Yeo, *Defining Science*, pp. 225–26.

45 *Report of the Third Meeting*, p. xxv. A very useful account of Whewell's views on technology and 'art' is given in Joost Mertens, 'From Tubal Cain to Faraday: William Whewell as a philosopher of technology', *History of Science*, **38**, 2000, 321–42.

46 J.B. Pentland to Watt Jr, [?] October 1839, as extracted in Watt to Muirhead, 22 October 1839, Muirhead Papers, MS GEN 1354/379.

47 J.P. Muirhead to James Watt Jr, 10 September 1839, Muirhead Papers, MS GEN 1354/310.

48 E.W. Harcourt (ed.), *The Harcourt Papers*, 1880–1905, vol. xiv, p. 97.

7.2 The Reverend William Whewell

was also publicly and privately supportive. His private response to Harcourt's Address and the stir that it created is worth quoting at length:

> I see that you are assailed on various sides for what you have said about Watt and Cavendish. I have no doubt that you will be able to defend yourself easily and well; and indeed I hardly see the necessity of adding anything to what you originally stated.
>
> Your case, as you first put it, remains to my mind quite unshaken. But perhaps you may be wishing to know how the subject presents itself to my mind, looking at it rather

> with reference to the general course of the history of Science than to any special evidence of dates and the like …
>
> In the first place I think your remark is quite decisive – that Watt's views are utterly damaged by involving a composition of ponderable and imponderable elements. This of itself was enough to show that he did not consider elementary composition with the rigour and distinctness which the discovery of the synthesis and analysis of bodies at that time required …
>
> And thus Watt, who at most can only claim the merit of proposing a hypothesis, proposed one which by its very terms was unsuited to the step which science then had to take.[49]

In this way, Whewell came to Harcourt's aid as the latter prepared the written account of the Birmingham Address with which we began. Whewell's public support came, significantly, in his best-known and most influential works, his *History of the Inductive Sciences* and his *Philosophy of the Inductive Sciences*, where the advice that he gave to Harcourt was developed to elevate the Cavendish camp's position to a matter of philosophical principle.

Whewell quickly found a place in his philosophy of science for the issues raised by the water controversy. In *Philosophy of the Inductive Sciences*, first published in 1840, Whewell discussed the application of the 'idea' of substance in chemistry and in the course of this developed a series of maxims. One of these was the 'Maxim respecting Imponderable Elements', that is, concerning those hypothetical fluids that had at one time or another been deployed to explain phenomena such as heat, light, electricity and magnetism.

> It is however plain, that so long as these fluids appear to be without weight, they are not elements of bodies in the same sense as those elements of which we have hitherto been speaking. Indeed we may with good reason doubt whether those phenomena depend upon transferable fluids at all … Consequently the maxim just stated, that in chemical operations nothing is created, nothing annihilated, does not apply to light and heat. They are not <u>things</u> … In reasoning respecting chemical synthesis and analysis therefore, we shall only make confusion by attempting to include in our conception the light and heat which are produced and destroyed. Such phenomena may be very proper subjects of study, as indeed they undoubtedly are; but they cannot be studied to advantage by considering them as sharing the nature of composition and decomposition.[50]

Given Whewell's argument that the proper pursuit of science involved the measurement of facts with precision, then chemical composition and decomposition must be studied by measuring the weights of the ingredients and compounds. Because there is no measure of any value in relation to heat and light, 'if we attempt to account for these phenomena *on chemical principles*, we introduce, into investigations themselves perfectly precise and mathematically rigorous, another class of reasonings, vague and insecure, of which the only possible effect is to vitiate the whole reasoning, and to make our conclusions inevitably erroneous'.[51]

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49 Whewell to Harcourt, 11 February 1840, in *The Harcourt Papers*, vol. xiv, pp. 105–106.

50 William Whewell, *The Philosophy of the Inductive Sciences*, 2 vols, 1840, vol. 1, pp. 399–400.

51 Ibid., p. 400. It will be noticed that Whewell's philosophical principles reflect rather directly in many respects Lavoisier's philosophical–rhetorical grounding of his new chemistry.

This led Whewell to the maxim that 'imponderable fluids *are* not *to be admitted as chemical elements of bodies*'.[52]

Whewell believed, then, that the best practitioners of chemistry, the 'most philosophical chemists', had proceeded according to this maxim. He proceeded to apply it to the controversy over the composition of water. Noting that, after a long period when the discovery was credited to Cavendish and Lavoisier, a claim had been made for James Watt's priority, Whewell quickly dismissed the usual terms of debate over the controversy: 'It is not our purpose here to discuss the various questions which have arisen on this subject respecting priority of publication, and respecting the translation of opinions published at one time into the language of another period.'[53] Quoting Watt's statement of his views, Whewell noted that Watt 'does admit imponderable fluids as chemical elements; and thus shows a great vagueness and confusion in his idea of chemical composition'.[54] According to Whewell, this flaw in Watt's approach meant that Watt did not understand, let alone anticipate, the discovery of Cavendish and Lavoisier. The conclusion drawn by Cavendish, on the other hand, Whewell considered to have contained 'nothing hypothetical or superfluous'. On this ground Whewell agreed with Harcourt's decision in favour of Cavendish. At the same time, he congratulated both Harcourt and himself: 'we may with pleasure recognise, in this enlightened umpire [Harcourt], a due appreciation of the value of the maxim upon which we are now insisting'. Whewell quoted Harcourt's observation that Cavendish had wisely 'pared off' imponderables 'as complicating chemical with physical considerations'.[55] That Harcourt was in accord with Whewell is, of course, hardly surprising since, as we have seen, Whewell had fed that line of argument to Harcourt earlier in the year!

Whewell's is a powerful argument. He took the philosophical high ground, established the right way of proceeding in science and found Cavendish in accord with it. Watt, by contrast, was adjudged out of line. Whewell was very clear about the philosophical test that a passage of scientific activity should pass before it could be regarded as a scientific discovery. This philosophical test drove a wedge between Cavendish and Watt and rendered circumstantial evidence irrelevant to the choice between them. Whewell made this clear in a note added to the discussion of the controversy in the second edition of *The Philosophy of the Inductive Sciences*, published in 1847:

> Since the first edition of this work was published, and also since the second edition of the *History of the Inductive Sciences,* Mr. Watt's correspondence bearing upon the question of the Composition of Water has been published by Mr. Muirhead. I do not find, in this publication, any reason for withdrawing what I have stated in the text above: but with reference to the statement in the *History*, it appears that Mr. Cavendish's claim to

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52 Ibid.

53 Ibid., p. 402.

54 Ibid.

55 Ibid., p. 403. There were differences, however, between Harcourt and Whewell. Harcourt thought that Whewell gave too much credit to Lavoisier. He believed that the Frenchman's representation of the phenomena had 'the same defect as that of Watt' in giving heat 'a part in the affinities & composition of the gaseous & liquid substances'. In Harcourt's view Cavendish alone avoided this. See Harcourt to Whewell, 16 March [1840], Whewell Papers, Add. Ms.a.205$^{138(1-2)}$.

> the discovery was not uncontested in his own time. Mr. Watt had looked at the composition of water, as a problem to be solved, perhaps more distinctly than Mr. Cavendish had done; and he conceived himself wronged by Mr. Cavendish's putting forwards his experiment as the first solution of this problem.[56]

Whewell made a minor concession to the Watt camp here. He conceded that Muirhead's publication of Watt's correspondence provided some insight. It showed us, as a purely historical matter, that Cavendish's claim was contested in its own time. It showed us also, perhaps, that Watt had a clearer focus than Cavendish did upon the problem of the nature of water. Finally it enabled us to understand how it was that Watt felt aggrieved by the lack of recognition granted him. However, the concession was only minor, because these historical insights were treated as irrelevant to the task of designating the discoverer. Whewell might acknowledge that at the time (in the 1780s) people had grounds for granting Watt's claim, but now, in the 1840s, with the correct philosophical test of the question available, there could be no doubt that Cavendish was the discoverer and Watt was irretrievably wide of the mark.

While Whewell traced the sources of Cavendish's success to the ability to conform to scientific inquiry pursued via clear and distinct ideas, others had a rather different emphasis. John Herschel, for example, had some well-known differences with Whewell's idealism and was inclined to emphasize the role of experiment. This is revealed not only in his *Preliminary Discourse on the Study of Natural Philosophy* (1830) but also in the way that he dealt with particular instances of discovery.[57] David Gooding has brought to light Herschel's dealings with Michael Faraday on the occasion of Faraday's announcement of his discovery of the relationship between light and electromagnetism – what we know as the 'Faraday Effect'.[58] This occurred in 1845, at a time when the water controversy was very active. Interestingly it involved a relationship between Herschel and Faraday that parallelled in some key respects that often posited between Watt and Cavendish. Basically, Herschel had an anticipatory idea in 1823 and performed an abortive experiment. He did not pursue the phenomenon any further experimentally. Faraday, in a private train of research, came up with the effect. Herschel's response to Faraday's claim was essentially to say that although he had done similar work, he did not claim priority. He acknowledged instead that Faraday had the stronger claim. Herschel summed his reasoning up nicely in a letter with the phrase 'He who proves discovers'. The idea is, then, that a person engaged in a train of research, a person who elaborates the work around a discovery, who performs a proving experiment, has a strong claim to that discovery. By the 1840s this idea had become a kind of informal social rule of what we might call 'priority politics'.

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[56] William Whewell, *The Philosophy of the Inductive Sciences*, 2nd edition, 2 vols, 1847, vol. 1, p. 419n.

[57] J.F.W. Herschel, *A Preliminary Discourse on the Study of Natural Philosophy*, 1830 and see Timothy L. Alborn, 'The "End of Natural Philosophy" revisited: Varieties of scientific discovery', *Nuncius: Annali di storia della scienza*, **3**, 1988, 227–50.

[58] David Gooding, '"He who proves, discovers": John Herschel, William Pepys and the Faraday Effect', *Notes and Records of the Royal Society of London*, **39**, 1985, 229–44.

Another example, in relation to an observational science, is provided by the attempt of James David Forbes to claim a discovery. In 1847 he reflected upon his own status as a discoverer in glaciology in a letter to his former pupil and friend E. Batten, who seems to have expressed more confidence in Forbes's claim to the discovery than Forbes himself thought warranted:

> You must allow me some voice in deciding whether the glacier question may safely be left to itself. Your repeated assurances that everyone is satisfied are very pleasing, as expressing a sort of public opinion; but I have the very best reasons for knowing that only a small portion of strict men of science, whose opinion must ultimately decide the matter, are convinced, or at least, if convinced have the candour to allow it. Dr Whewell has spoken out manfully in the new edition of his 'Inductive History', and I hope Lyell will do as much, and then we may expect others to follow. But the result of my reading in the history of science is that in questions of mixed evidence like this a man must work it out to the utmost limit he can, if he means that it shall finally be associated with his name, which is my desire …[59]

The young Forbes had been given advice by David Brewster to specialize in a train of research and he had followed that advice.[60] It is perhaps not surprising that, for a generation becoming increasingly specialized in their own careers, which they envisaged in terms of trains of research, this criterion of discovery should be a popular one. Cavendish would be favoured by this criterion much more so than Watt, since the accepted history portrayed Cavendish, unlike Watt, as an ongoing experimental presence in the emergence of the new chemistry. Forbes's judgement of where priority lay in the water controversy also informed his own mode of scientific life as a putative discoverer. Forbes's generation was one that increasingly pursued a single line of research. Forbes did just that, first with studies of heat and then with studies of glaciation. It is little surprise, then, that for that generation Cavendish's sustained researches were admired and seen as a ratification of the modern order. The road to discovery taken by the new generation was predicated on the judgement that in the water controversy Cavendish was the true discoverer.[61]

Although in some respects their own careers informed their notions of discovery, a key strength of the Cavendish camp in the water controversy lay in the capacity to incorporate their case into a general, objectified philosophical outlook upon science that was widely promulgated. Another strength lay in the closely related task of

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[59] James David Forbes to E. Batten, 10 January 1847, in John Campbell Shairp, Peter Guthrie Tait and A. Adams-Reilly, *Life and Letters of James David Forbes, F.R.S.*, 1873, pp. 180–81.

[60] See Shairp et al., *Life and Letters of James David Forbes*, p. 46–47.

[61] Forbes's and Herschel's views emphasized the hard work done in connection with discovery. For them this aspect of the process of discovery became an important criterion for determining whether and by whom it had been achieved. This contrasted with the emphasis in Whewell's philosophy upon the role of genius and sudden inspiration. Forbes put it this way to Whewell: 'Perhaps there is some fundamental difference between us on the subject of Practical Knowledge … Of the necessity of patient thought to the elaboration of anything of value I am so strongly convinced that I can hardly admit knowledge suddenly acquired to having much intrinsic worth … ' (Forbes to Whewell, 29 October 1848, Whewell Papers, Add. Ms.a.204[84]). On some of these differences in conception of discovery and the role of genius see Patricia Fara, *Newton. The Making of Genius*, 2002, pp. 222–30.

controlling historical interpretation. This was truly a collective task. The initial production and updating of Whewell's *History of the Inductive Sciences* was a collaborative effort in which Whewell consulted widely, particularly on recent science. Whewell and his collaborators served a self-appointed judicial function. Priority in the water question was just one of the numerous issues that Whewell pronounced upon. He was careful to consult and to build up a consensus around his decisions. Whewell was acutely aware that judgements about what to include in the history, and whom to credit with significant discoveries, were very delicate matters.[62]

Forbes faced similar issues of judgement a few years later when he came to write the historical 'Dissertation' for the eighth edition of the *Encyclopaedia Britannica*. In fact, in negotiating the fee for the job with the editor, Dr T.S. Traill, Forbes put a price on the exercise of his judiciousness, explaining that the task was very difficult:

> (1) On account of the immensely scattered literature of modern science (2) on account of these contributions not having yet taken a decided rank in the History of Science, so that the compiler must himself give a judicial opinion upon their merits in many cases (3) on account of the obvious delicacy of dealing with contemporary or almost contemporary operations.[63]

Forbes gained a substantial fee, took the task on, and three years later Whewell sympathized with his colleague:

> I know that it requires some effort to act in such a case with perfect impartiality, when one knows that what one has to say will disappoint worthy and laborious people: but the feeling of the value of historical truth supports one under such annoyances. I cannot think otherwise than that your book will be well received: though perhaps not by that class of popular readers who have been led to believe that they can judge of scientific discoveries without knowing anything of science: an impression studiously strengthened by various persons, some of whom know no better, and others ought to …[64]

There can be no clearer claim by Forbes, Whewell and their ilk to the right to arbitrate the history, even the contemporary history, of scientific discovery. Nor could there be any clearer denial of that right to the scientifically uninitiated.[65] Brougham's contributions to debate on the water question had led Harcourt to publicly advise the noble Lord that 'those who have not had leisure to pursue chemical studies to their foundation' should 'leave chemistry and chemists to themselves – at least so far as regards the minutiae of the science, and arbitrations of the rights of discovery … '.[66] There is no doubt that part of the opposition to the

[62] For examples of Whewell's consultations see: Airy to Whewell, 19 June and 16 November 1846 (Whewell Papers, O.15.48[4] and O.15.48[10]); Forbes to Whewell, 24 September, 26 September and 13 October 1846 (Whewell Papers, O.15.48[24–28]).

[63] J.D. Forbes to T.S. Traill, 10 March 1852, Forbes Papers, Incoming Letters 1852, 25a.

[64] Whewell to Forbes, 10 March 1855, Forbes Papers, Incoming Letters 1855, 40a.

[65] For Whewell's campaign against much scientific journalism and its links with his project of linguistic reform, see Simon Schaffer, 'The history and geography of the intellectual world: Whewell's politics of language', in Menachem Fisch and Simon Schaffer (eds), *William Whewell: A Composite Portrait*, 1991, pp. 201–31.

[66] William Vernon Harcourt, 'Letter to Henry Brougham', *Philosophical Magazine*, **28**, 1846, 524.

claims for Watt in the water question was grounded in the view that both Watt himself and his advocates lacked the requisite scientific understanding to be *bona fide* contributors either to scientific discovery or to its historical elucidation. Not only this, but they failed to recognize what Whewell took to be a historic truth, namely that the superseding of natural philosophy by science, of eighteenth-century modes of inquiry by modern ones, was exactly the exclusion of the inexpert from the councils of judgement. Writing about electrical investigations in the eighteenth century Whewell argued that

> A large and popular circle of spectators and amateurs feel themselves nearly upon a level, in the value of their trials and speculations, with more profound thinkers: at a later period, when the subject is become a science, that is a study in which all must be left far behind who do not come to it with disciplined, informed and logical minds, the cultivators are far more few, and the shout of applause less tumultuous and less loud … [67]

The appeal of the British Association to the likes of Whewell was in part that it offered an ideal vehicle through which to manage the relations of experts and the lay public. Harcourt's Address before that body in 1839, then, was not a lone intervention in the history of science. It was written from the perspective of a scientific grouping whose members were heavily engaged in shaping history. They did so partly in the cause of developing a particular set of institutional frameworks and creating science in their own image.

In a real sense, then, Harcourt's 'Address' as published in 1840 was a common effort of Harcourt and his supporters in the months after his Birmingham speech. Supporters were drawn from among those Gentlemen of Science who commanded the affairs of the British Association and, increasingly, the Royal Society during these years, as well as from the Cavendish family itself. The water question was one issue in a much larger ideological struggle about the nature of science, its relations with other major institutions, and its role in society. The extent of this larger struggle can be further appreciated by considering another individual who was heavily involved in the water controversy but also fought numerous other battles with the Association's leadership – Sir David Brewster. His case is instructive not only because he changed sides in the water controversy but also because, unlike the 'foundation' members of the Watt camp, he also fought explicitly, and for many years, over the larger symbols of science.

Brewster's Intervention and Conversion

In the interval between the delivery of Harcourt's address to the Association and its publication another important document appeared. This was the review of Muirhead's translation of Arago's *Eloge* in the *Edinburgh Review* in January 1840.[68] The Watt

[67] William Whewell, *History of the Inductive Sciences*, third edn, 1857, vol. 3, p. 16, as quoted in Simon Schaffer, 'Scientific discoveries and the end of natural philosophy', *Social Studies of Science*, **16**, 1986, 407.

[68] David Brewster, 'Life and discoveries of James Watt', *Edinburgh Review*, **70**, 1840, 466–502.

camp had assumed, complacently, that Brougham would be 'their' reviewer in the *Edinburgh* simply by virtue of seigniorage. However, Macvey Napier, the editor, had already assigned the task to David Brewster, though he left Muirhead, Watt Jr and Brougham guessing as to the reviewer's identity. Their concern that the *Edinburgh* might not be automatically supportive increased when, in response to news reports of Harcourt's British Association speech, Brougham tried to insert a critical note into the *Review*. In enclosing the intended note, Brougham suggested that Harcourt had been appealing to 'vulgar national feelings' at the British Association 'running his lengths, and all because Watt was a Scotchman, and Arago is a Frenchman'. Napier refused to publish the note.[69]

Brewster's review perplexed those on both sides of the water question. He argued that the credit should be distributed between Watt and Cavendish, credit for the *theory* going to Watt but credit for the *discovery* to Cavendish. Brewster suspected that his stance would not be popular. His situation between the camps on the substantive question mirrored his situation in life. Brewster was, in many respects, one of the 'Gentlemen of Science' who had founded, and ran, the British Association. He had been, however, a continual thorn in the side of the Cambridge men almost from the beginning and was at loggerheads with them, especially Whewell and Airy, on many fronts. Brewster regarded himself as a friend of Watt and of Scottish philosophers and inventors generally. Yet, on the water question, Brewster had proved a disappointment to the Watt camp. He had been shown Watt's water correspondence but claimed to be unconvinced by it. As editor of the *Edinburgh Encyclopaedia* he reprinted sections of Watt Jr's *Britannica* article on his father but appended a comment expressing scepticism about the claim to the discovery of the composition of water. On the strength of this alone, by the time the controversy was fully joined in 1839–40, Watt Jr regarded Brewster as one of the enemy.

Brewster's involvement, however, took an interesting twist. In 1846 he published his second major contribution to the controversy, this time unequivocally in Watt's favour. Brewster's 'conversion' was a surprise to the Watt camp, and they treated it

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69 Brougham to Napier, 22 September 1839, in Macvey Napier Jr (ed.), *Selection from the Correspondence of the late Macvey Napier Esq*, 1879, pp. 300–301. The text of the unpublished note read: 'Want of room compels us to postpone, to our next Number, a notice of a late address, by a worthy and reverend individual, at the Birmingham general meeting for scientific purposes. This address undertakes to decide, and somewhat peremptorily does assume to decide, upon a question of great scientific interest, namely, Mr. Watt's claims to be regarded as the first discoverer of the composition of water, that is (for no one claimed more for him) to have, in point of time, though unknown to Mr. Cavendish, made that important step. M. Arago, in his admirable memoir of Watt, and Lord Brougham in his dissertation, inserted by M. Arago in that memoir, having distinctly stated the evidence, which is that of dates and documents, Mr. Vernon, not satisfied with the scientific powers of one of these academicians, or the powers of the other to deal with evidence, has somewhat dogmatically denied the whole of their inferences, and made an appeal of a somewhat popular cast against the claims of our countryman. The whole case shall be told in our next Number, both from the documents now before the world, and from others, of much importance, to which we have had access. It is enough for us to state at present that Mr. Vernon's whole theory rests on an assumption of fact absolutely groundless, and contrary to all the evidence, namely, that Dr. Priestley did not, until taught by Mr. Cavendish, ascertain that the weight of the water formed by the combination of the two gases is equal to the weight of the gases. This we undertake to prove wholly untrue, from all the evidence published and unpublished.'

with some caution. Cavendish's supporters observed with knowing condescension. All their prejudices about Brewster as fickle, wrong-headed and contrary in all his dealings were confirmed. So, who was David Brewster? What were his arguments on the water question? Why did he take the stances that he did? Why did he change his mind? We will find answers to many of these questions in the ideological struggles of early nineteenth-century British science, scarce one of which did not enjoy the benefit of Brewster's intervention.

Brewster was the son of the headmaster of Jedburgh Grammar School and proceeded to Edinburgh University to study divinity.[70] At Edinburgh he was a contemporary and friend of Henry Brougham, who was to be a lifelong ally, collaborator and patron. Though originally intended for the Church, Brewster did not follow that path, it is said because of a nervousness that interfered with public speaking. This nervousness was often remarked upon and, though not always debilitating – Brewster gave many fine speeches – it does appear to have blighted his later professorial ambitions.

The career upon which Brewster launched himself early in the new century was the exciting but precarious one of a writer and editor. He held no paid appointment of any kind until 1838, when he became Principal of St Andrews, although he did spend time from the mid-1830s as manager of an estate owned by his sister-in-law. He commenced as editor of the *Edinburgh Magazine* and then of the *Scots Magazine* until 1806 or 1807. He then began the task of editing the *Edinburgh Encyclopaedia* for the publisher Blackwood, a job not completed until 1830. Brewster wrote for other reviews, magazines and encyclopaedias and persuaded Constable to support *The Edinburgh Philosophical Journal*, which he edited jointly with Robert Jameson. They had a very tempestuous relationship. Brewster was always an eager and dogged controversialist. By the mid-1820s, the scientific journal was caught in legal wrangles that resulted in a split and various transformations over the years.[71]

Brewster pursued his scientific work alongside this precarious literary career. Beginning in 1806, he had conducted research especially in experimental optics and optical instruments. The years 1810–18 were his most productive period of research. This brought him a strong international reputation as an experimental natural philosopher. He was also the author of a number of patented inventions, including the kaleidoscope. Though very productive scientifically and a talented and entertaining writer with a growing stable of books to his credit, Brewster was, by the late 1820s, an anxious and frustrated man. He saw that to gain financial stability he needed a secure appointment. Professorships eluded him. Their rewards in the Scottish system could be substantial if large numbers of students could be attracted. Those who did succeed, Brewster denigrated, with some justice but equal measure of envy, as showmen who had little time for research or success in it. Though a professorship would be financially desirable, it would interfere with his research. Brewster's ideal would be a position in which an assistant could discharge the lecturing duties. This was the stance that he took when competing for the

[70] On Brewster see Mrs [Margaret Maria Brewster] Gordon, *The Home Life of Sir David Brewster*, 1869.

[71] See W.H. Brock, 'Brewster as a scientific journalist', in A.D. Morrison-Low and J.J.R. Christie (eds), '*Martyr of Science*'*: Sir David Brewster 1781–1868*, 1984, pp. 37–42.

Edinburgh Chair of Natural Philosophy in 1832–33. He was beaten to that post by his young protégé, James David Forbes, whose family influence, Tory politics, considerable promise and testimonials from luminaries of Cambridge gave the Town Council one excuse too many to reject the highly reputable, but nervous and disputatious, Whig, Brewster.

Brewster pursued a number of plots with his old friend Henry, Lord Brougham, who by the early 1830s was the highest legal officer in the land as Lord Chancellor and a man of considerable influence and substantial patronage. Brougham tried to help Brewster (an evangelical Presbyterian) gain ordination and a living in the Church of England, but in the end nothing came of it. A knighthood in 1831 and a government pension of £100 per annum, increased to £300 in 1836, were tangible outcomes of Brougham's patronage but still not the sufficiency that Brewster sought.[72]

There is no need to doubt Brewster's conviction as to the benefit that would flow from his schemes for the reform of scientific institutions despite their links with his personal frustrations.[73] In the late 1820s Brewster became known, alongside Charles Babbage, as a leader of those who argued that science in Britain was in decline. He railed against the government for its failure to honour scientific men, or, more importantly, to create salaried positions for them. He consistently held up the French state's support of science and of scientists as a contrast. Unreformed scientific organizations and the ancient universities were another target because of their failure to promote scientific research more effectively. The patent laws, in Brewster's view, penalized and taxed inventors rather than rewarding and encouraging them.[74]

The idea of creating a new scientific association was designed, in Brewster's mind at least, to address all these issues. The contrast between the body that Brewster envisaged and the one that was created as the BAAS is largely accounted for by the influence of the Cambridge Network, whose clerical members in particular disagreed with Brewster's vision and resented his flailing criticism of their own beliefs and activities. A number of authors have characterized this culture clash, personified in the antagonism between Brewster and Whewell.[75] None has done it so well as John Hedley Brooke. In the course of his examination of the Brewster–Whewell debate on the plurality of worlds, Brooke explored the sources of animosity between them.[76]

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[72] See Brewster to Brougham, 22 July 1829, 21 January 1832, 9 May 1832, 28 May 1832, in Brougham Papers, 26,609; 26,615; 15,728 and 26,616.

[73] See Steven Shapin, 'Brewster and the Edinburgh career in science', in Morrison-Low and Christie (eds), '*Martyr of Science*', pp. 17–23, who sees Brewster's campaign to reform British science as deriving from his 'personal predicament, joined to his moral conception of the proper place of science in Christian civilization' (p. 21).

[74] David Brewster, 'The decline of science in England', *Quarterly Review*, **43**, 1830, 305–42.

[75] Morrell and Thackray, *Gentlemen of Science: Early Years*, pp. 345–48.

[76] John Hedley Brooke, 'Natural theology and the plurality of worlds: Observations on the Brewster–Whewell debate', *Annals of Science*, **34**, 1977, 221–86. In relying on Brooke's exemplary depiction of the personal struggle between Whewell and Brewster that was built up through political, methodological and substantive scientific disputes, I acknowledge that Brooke ultimately regards their theological differences as the most fundamental, underlying even their methodological and substantive scientific stances. On Brewster's theological views and involvement in Scottish religious affairs see Paul Baxter,

Brewster and Whewell initially fell out over one of Brewster's contributions to the decline of science debate in the *Quarterly Review*. In this he suggested, tactfully as ever, that there was nobody 'in all the eight universities in Great Britain who is at present known to be engaged in any train of original research'.[77] Brewster had a scheme to divide the richer professorships and employ on the one hand a 'gifted philosopher', who would pursue his research, and on the other a popular lecturer, who would attend to the needs of students. This also caused offence in Cambridge. Whewell remained unconvinced of the declinists' case and therefore, not surprisingly, of their remedies also. One of these was, as we have seen, the new scientific association that Brewster early conceptualized. Whewell and other Cambridge men (with the exception of Babbage) were against joining with an association based on Brewster's ideas: 'I should feel no great wish to rally around Dr. Brewster's standard after he had thought it necessary to promulgate so bad an opinion of us who happen to be professors in universities … It requires all one's respect for Dr. Brewster's merits to tolerate such bigotry and folly.'[78] When Harcourt steered the Association along lines more congenial to the Cambridge men, Brewster naturally complained at the directions that 'his' Association had taken, and held the 'contingent from Cambridge' explicitly responsible for its waywardness.[79]

By this point it was inevitable that Whewell's *History of the Inductive Sciences* would meet with Brewster's jaundiced eye. He ridiculed Whewell's neglect of the practical achievements of genius, and of Scotsmen.[80] Whewell's project continued in the *Philosophy of the Inductive Sciences* published in 1841, to which Brewster also gave a hostile reception in the pages of the *Edinburgh*. Whewell, in Kantian terms, favoured the prepared mind as the vehicle by which facts 'became the materials of exact knowledge' and, accordingly, declared against the role of chance in discovery. Brewster, closer than Whewell to the empirical view of discovery, cited examples where accident had played a major part, and accused Whewell of having his head in the clouds of an irrational metaphysics and his book of having little relevance to physical science as such.[81] Brewster considered that his own researches had received little recognition in Whewell's *History* or in his *Philosophy*. This hardly surprised him, given the long tussle that he had engaged in with the Cambridge men on the question of theories of light. In 1841, even as he was reviewing the *Philosophy*, Brewster was in dispute over the refusal of the Royal Society to publish one of his optical papers, a rejection in which Airy and Whewell had played a major role.[82]

'Science and belief in Scotland, 1805–1868: The Scottish evangelicals', unpublished PhD thesis, University of Edinbugh, 1985.

77 Brewster, 'The Decline of Science in England', 326–27.

78 William Whewell to J.D. Forbes, 14 July 1831, Forbes Papers, Incoming Letters, 1831, no. 25.

79 David Brewster, 'Report of the first, second and third meeting of the British Association', *Edinburgh Review*, **60** (1835), 363–94, at 374–82.

80 David Brewster, 'Whewell's *History of the Inductive Sciences*', *Edinburgh Review*, **66**, 1837, 110–51.

81 David Brewster, 'Whewell's *Philosophy of the Inductive Sciences*', *Edinburgh Review*, **74**, 1842, pp. 266, 292–96.

82 On this, see G.N. Cantor, 'The reception of the wave theory of light in Britain: A case study illustrating the role of methodology in scientific debate', *Historical Studies in the Physical Sciences*, **6**,

Beyond even this specific point, though, was Brewster's undoubted bitterness that a man such as Whewell should be sitting in judgement upon him. Whewell's magisterial manner and leisured omniscience (in the absence of any particular, original contribution to scientific knowledge) stuck in the hardworking Scotsman's craw. It must have seemed to him the worst manifestation of a process whereby the Cambridge men's supererogatory mathematical theories were treated as if they were immune to his experimental findings. This view was also consonant with Brewster's aversion to the philosophy of Bacon. There were many versions of Baconian philosophy touted in mid-nineteenth-century Britain and they were used as flexible rhetorical resources. Brewster, however, was inclined to reject any of these uses because he considered Bacon's precepts as failing to engage with the material realities of scientific practice. Whewell's announced self-conception as the writer of a modern *Novum Organon* spoke to Brewster of the same problem.[83]

From this ongoing antagonism one would expect that Brewster's inclination in the water controversy would be very much against Cavendish and for the claims of Watt. However, the situation was not so simple. Let us trace the history of Brewster's relations with the Watt camp before turning to an analysis of his major contributions to the debate on the water question.

Brewster and the Filial Project

We have seen that the filial project pursued by Watt Jr was begun by his father in the years before his death. Watt was not averse to making his own contribution to the historical record where it was considered necessary. The way that the response to Olinthus Gregory was arranged through the *Edinburgh Review*, with Watt father and son active behind Playfair as author, indicates their style of intervention. Brewster became involved in another opportunity for the Watts to set 'straight' various matters regarding the steam-engine innovations. This was done in the form of Watt's notes on, and corrections to, the articles 'Steam' and 'Steam-Engines' written by Robison for the third edition of the *Encyclopaedia Britannica*. These notes and corrections were published in 1822 as part of Brewster's edition of John Robison's *A System of Mechanical Philosophy*.[84]

1975, 109–32; idem, *Optics After Newton*, 1983, pp. 175–76. For the scientific collaboration between Brewster and Brougham see Xiang Chen and Peter Barker, 'Cognitive appraisal and power: David Brewster, Henry Brougham, and the tactics of the emission–undulatory controversy during the early 1850s', *Studies in History and Philosophy of Science*, **23**, 1992, 75–101.

[83] On varieties of 'Baconianism' at this time and a persuasive discussion of the uses made of them see Richard Yeo, 'An idol of the marketplace: Baconianism in nineteenth-century Britain', *History of Science*, **23**, 1985, 251–98. Yeo explains, along the lines suggested here, why Brewster's strong emphasis upon experiment and his interest in applied science and technology, which would normally be considered as the hallmarks of someone inclined to identify with Bacon, did not have that association in his case.

[84] John Robison, *A System of Mechanical Philosophy*, 4 vols, 1822. Watt circulated some separate copies of the amended articles and other material earlier: *The Articles on Steam and Steam-Engines, written for the Encyclopaedia Britannica, by the late John Robison … With Notes and Additions by James Watt … And a letter on Some Properties of Steam, by the Late John Southern*, 1818.

On Robison's death in 1805 his family had asked John Playfair to write a biography and to edit Robison's *Britannica* articles for separate publication.[85] Playfair completed half the job; the other half was entrusted to Brewster. Although Watt made various statements implying that his work on the two steam-related pieces was a chore,[86] it seems more likely that it was gladly seized as another opportunity to set the historical record 'straight'. Watt Jr and John Southern assisted Watt in the task during 1813–14. Over the next few years until the work was finally published, Brewster, as editor of the overall project, was a presence in the Watt family's literary affairs. He encouraged Watt to clarify his relations with Joseph Black, pointing out that authors were often in the habit of attributing the steam-engine improvements directly to Black's discovery of latent heat. Brewster also sought, and received, advice from Watt about the patenting and manufacture of his invention, the kaleidoscope. Their correspondence at this time reveals a close relationship, the younger man clearly revelling in their kindred scientific and technical interests.[87] During this, the most scientifically productive phase of his life, Brewster appears to have regarded Watt as a friend and fellow discoverer and inventor. One is tempted to say that Brewster identified with the much older man. It was perhaps for this reason that Watt Jr, after his father's death and the 'discovery' of the water correspondence, included Brewster among those whom he consulted about the import of that correspondence. It would not have pleased Watt Jr that Brewster, along with Hope, was unpersuaded of his father's priority.[88]

When, in 1830, Brewster's *Edinburgh Encyclopaedia* reached volume 18, in which the article on James Watt was to appear, he quoted in full the section of Watt Jr's *Britannica* article dealing with the composition of water and including the footnote on confusion of dates. Brewster, however, chose to append some remarks in which he challenged the article's claims on the water question:

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85 The following relies on W.A. Smeaton, 'Some comments on James Watt's published account of his work on steam and steam engines', *Notes and Records of the Royal Society of London*, **26**, 1971, 35–42.

86 See, for example, Watt to Sir Joseph Banks, 1 March 1815, Royal Society Library, BLA.W.17, as printed in Robinson and McKie, *Partners in Science*, pp. 419–21. Watt states: 'at the instigation of my friends, and to my own great annoyance, I spent all my working hours of the winter of 1813 and spring of 1814 in writing a commentary on my Friend Dr. Robison's memoir upon the Steam Engine in the *Encyclopaedia Britannica*, for the use of Dr. Brewster … '.

87 See Brewster to Watt, 25 August and 10 December 1815, James Watt Papers (Doldowlod), C6/3. Watt and Brewster were both independently involved in the invention of an improved micrometer with a moveable object-glass only to discover that they had both been anticipated by an earlier inventor, De la Hire. This led to an interesting exchange on the nature of discovery. Brewster's view was that 'an invention which is not communicated to the world is considered as not having been made till the time of its publication. This is the doctrine which has, for a long time, been held both with respect to inventions and discoveries; for it is always to be presumed that a person who does not communicate an invention to the public, does not see its full value, and does not consider it of any public utility' (Brewster to Watt, 12 December 1816, Watt Papers (Doldowlod), C6/4). See also J.P. Muirhead, *The Life of James Watt*, 1858, pp. 233–34.

88 Brewster recalls consultation about the correspondence in 'Observations connected with the Discovery of the Composition of Water', *London, Edinburgh and Dublin Philosophical Magazine*, **27**, 1845, 195–97, and it is recorded by Watt Jr in his prefatory letter to J.P. Muirhead (ed.), *The Correspondence of the late James Watt*, 1846.

> We have copied the preceding statement as that of Mr. Watt's friends; but a regard for the reputation of Mr. Cavendish, independent of higher motives, compels us to acknowledge that the statement is partial, and the argument not well founded. We are not able at present to refer to the original documents, but we had occasion some years ago, along with a distinguished chemist, to examine them with minute attention, and it was then our decided conviction, that the merit of the discovery of the composition of water belonged to Mr. Cavendish.[89]

Through the 1830s Brewster retained this view. When Arago attended the Edinburgh meeting of the British Association in 1834, he consulted not only with Watt Jr but also with others, including Brewster, about the *éloge* of Watt that he was preparing. Arago reported his conversation with Brewster succinctly: 'Mr. Brewster que j'ai questionné au sujet de son article Watt de l'Encyclopédie d'Edimburgh, ne m'a rien dit qui puisse infirmer l'opinion que j'ai adoptée.'[90]

In 1839 Brewster was approached by Macvey Napier to review Arago's *Eloge* for the *Edinburgh Review*. As we have seen, the Watt camp had assumed that Brougham would do the job, as had the great man himself. Napier, however, was inclined to resist Brougham's imperious and presumptuous commands.[91] From Napier's point of view, Brewster had become a regular, reliable reviewer on scientific and technical subjects, whose work was often admired. Watt Jr considered Brewster to be 'already compromised by a hasty opinion given some years ago, which would appear to render him an unfit person'. To leave little to chance, Muirhead briefed Jeffrey, who then saw, and wrote to Napier to 'indoctrinate' him. The clear intent was to get at the reviewer through the editor to prevent erroneous views being propagated on the water question.[92]

Brewster took his work on the review very seriously. He spent several days going over the history, and bestowed 'great labour' on it.[93] He was relieved at Napier's favourable response to the completed article: 'I was literally terrified that you would be dissatisfied with the view I had taken of the Water question.' He felt a great sense of responsibility since the more he studied the subject, 'the more I was convinced that, with all my enthusiasm for Watt, both as a friend whom I loved, and as a countryman whom I worshipped, I must take such a view as would not

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89 'Watt, James', *The Edinburgh Encyclopaedia*, 20 vols, 1830, vol. 18, p. 786. (Reprinted by Routledge in 1999 under the editorship of Richard Yeo.)

90 Arago to James Watt Jr, 22 September 1834, James Watt Papers (Doldowlod), W/10. Watt Jr's annotation on this letter, summarizing its contents, states: 'Has questioned Brewster who gives a lame Acc^{t}.'

91 On Brougham's estrangement from the *Edinburgh Review* under Napier, see Joanne Shattock, *Politics and Reviewers: The* Edinburgh *and the* Quarterly *in the early Victorian Age*, 1989, pp. 34–38.

92 Watt Jr to Henry Brougham, 17 October 1839, Brougham Papers, 20,133; Muirhead to Watt Jr, 11 October 1839, Muirhead Papers, MS GEN 1354/357. For Jeffrey's intervention see Jeffrey to Napier, 20 October 1839 in Macvey Napier Jr (ed.), *Selection from the Correspondence of the late Macvey Napier*, 1879, pp. 303–5. See also Muirhead to Watt Jr, 24 October 1839 (MS GEN 1354/384) with its advice that Napier, thanks to Jeffrey, will be ascertaining Brewster's intentions in the review and if need be altering them.

93 Brewster to Napier, 4 November 1839, Napier Papers, British Library Add. MSS 34,621, ff. 470–71 and 10 December 1839, ff. 537–38.

altogether satisfy his unreasonable and uninstructed friends'.[94] Probably feeling the pressure from the Watt camp from behind the scenes, Brewster reported that he had decided to write to Watt Jr asking point blank whether he and his friends considered his father to have 'actually discovered' the composition of water, and if so on what grounds they believed this. Watt Jr's rather terse response was that he claimed for his father 'the theory of the composition of Water' and that the grounds were as stated in the *Supplement* to the *Encyclopaedia Britannica*.[95] Had Watt Jr claimed the *discovery* for his father, Brewster would have given up the article, but 'as the explicit answer … omits all mention of discovery, and states that he claimed only the theory, I felt myself relieved from embarrassment'. This gave Brewster a way out of his dilemma, which we will analyse in a moment. He was convinced that the scientific credibility of the *Edinburgh Review* was at stake, as well as his own:

> Had the Review contained an article making Watt the discoverer of the composition of Water, and taking that honour from Cavendish, you would have had thundering replies from the Duke of Devonshire and the Earl of Burlington; and I have no doubt that every chemist in Great Britain would have exploded their fulminating powders against the Review. If Mr. Watt is a wise man, he will write no more on the subject. If I had been a lawyer retained by the Cavendishes, I could make out a very good case to show that Watt himself placed no value on his hypothesis, and did not intend that any claim should be set up in his name in relation to the composition of Water.[96]

Brewster clearly considered the matter settled largely in Cavendish's favour, but with the hope that he had said enough about Watt's independent merit to placate Watt's supporters and to salve his own conscience as a friend of Watt. Let us now examine Brewster's argument in the 1840 article.

The article reviewed the three published versions of Arago's *Eloge*. Brewster early asserted Watt's scientific reputation by stating that the 'succession of inventions and discoveries' that constituted his improvement of the steam engine were 'deduced from the most profound chemical knowledge, and applied by the most exquisite mechanical skill'. He then devoted twenty pages to the history of the steam engine, a sketch of Watt's life, his engine improvements and the problems with patents. Brewster lamented, along with Arago, the time that Watt spent as a civil engineer, unable as he was at that time to capitalize on his patent:

> may we not add our astonishment, that civilized states should still persist in shackling, by bad laws, the freedom of inventive genius, and withholding from the best benefactors of their country, those inalienable rights which are conceded to every other member of

[94] Brewster to Napier, 7 January 1840, Napier Papers, British Library Add. MSS 34,621, ff. 2–4, printed in Macvey Napier Jr, *Selection from the Correspondence of the late Macvey Napier Esq*, 1879, pp. 314–15.

[95] Ibid. That this exchange between Brewster and Watt Jr did occur is confirmed by Watt Jr to Muirhead, 7 December 1839, Muirhead Papers, MS GEN 1354/444.

[96] Brewster to Napier, 7 January 1840, in Macvey Napier Jr, *Selection from the Correspondence of the late Macvey Napier Esq*, pp. 314–15. The legal case that Brewster hypothesized here would probably have been based on the fact that Watt, when annotating the articles 'Steam' and 'Steam-Engine', did nothing to question the attributions regarding the composition of water that they contained.

> the community? Had Mr. Watt been able to communicate his inventions to the public, and yet retain the same right to them that an author does to the productions of his pen … our country might have stood even higher than she does in the scale of nations.[97]

This is, of course, a direct reference to the debates about patent law reform that Brewster played a part in. He was not impressed by Muirhead's effort to exculpate Edmund Burke's opposition to the 1775 Act that had granted Watt and Boulton an extension of twenty-five years' protection. It will be recalled that Arago had singled out Burke's opposition to this measure for mention. Muirhead had pointed out, by way of excuse, that Burke was merely representing the interests of a constituent. Brewster regarded this as compounding, not excusing, Burke's 'crime'. If Burke had opposed the Act on principle, being against the granting of monopolies, then Brewster could have respected Burke's motive while regretting his ignorance. To oppose it in order 'to gratify the illegal cupidity of one man [the constituent]' Brewster regarded as indefensible. He urged statesmen of the present day to consider the higher good that would flow from proper recognition of 'intellectual rights' and not allow pressure from the selfish motives of immediate political supporters to cloud their judgement.[98]

Turning, finally, to the water controversy, Brewster made clear immediately his desire for compromise. He referred to it as a 'painful controversy' to be approached with delicacy, and he expressed 'an anxiety which we are sure our readers will share, to allay feelings which should never have been roused, and to reconcile interests which in the equipoise of justice are not at variance'.[99] Brewster's argument ran as follows. There is no doubt that on the basis of experiments performed by others Watt arrived at his hypothesis first. Important as this is, Brewster stressed its limitations, which he claimed were acknowledged by Watt himself. Watt never claimed, according to Brewster, to have *discovered* the composition of water. Quoting a passage from Watt's 1784 paper in the *Philosophical Transactions*, Brewster summarized thus:

> Mr Watt speaks of his hypothesis as a plausible conjecture, which might be refuted by subsequent experiments: and as he never wrote another word on the subject, nor made a single experiment after this paper was printed, how is it possible to identify this hypothesis with the discovery of the composition of water?[100]

Brewster interpreted Black in his lectures as agreeing with this, since Black saw Watt as the first to have the idea. However, Brewster remarked that Cavendish was 'the first who gave it solid foundation and credibility'. Brougham too, according to Brewster, avoided using the straightforward term discovery to apply to Watt's actions. In his Appendix to Arago's *Eloge* Brougham consistently referred to Watt's discovery of the *theory* of the composition of water. So, Brewster concluded, Brougham, Black and Watt himself only ever saw Watt as conjecturing or suggesting

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97 David Brewster, 'Life and Discoveries of James Watt', *Edinburgh Review*, **70**, 1840, 466, 478.
98 Ibid., p. 480.
99 Ibid., p. 488.
100 Ibid., p. 494.

that water was composed of oxygen and hydrogen. Cavendish, by contrast, 'proved by infallible experiments, that water consisted of oxygen and hydrogen, and therefore discovered its composition'.[101] Arago, however, 'applies the terms *hypothesis*, *theory*, and *discovery*, indiscriminately' and sometimes he gave Watt all the credit; at other times he gave much of it to Priestley.

> In this way the glory of having discovered the composition of water – that is, of having established it as a *physical truth* – is transferred in *small* shares to a joint-stock company, and *not one of these* is given to Mr. Cavendish! Mr. Watt himself speaks of 'his contempt for the modicum of reputation which would result from his own theory'.[102]

Whilst Watt wanted to claim what was due to him, he would have been astonished, Brewster claimed, if the hypothesis had been made to supersede Cavendish's 'grand experimental discovery'. So Brewster found that the merits of Watt and Cavendish were not in collision.

> Mr. Watt will for ever enjoy the honour of that singular sagacity which presented to him the *hypothesis* of the composition of water; and Cavendish will never lose the glory which belongs to him, of having given that hypothesis, whether he was cognisant of it or not, the force and stability of truth.[103]

Brewster, having established this point, moved on to castigate Harcourt for his treatment of the question, and the Council of the British Association more generally. Harcourt did injustice to Watt's genius in a careless way at the most inappropriate of venues. Brewster linked this to the recent efforts of another member of the Council to deprive Black of credit for the discovery of latent heat. (This was a reference to Whewell's *History of the Inductive Sciences* in which he attributed the discovery to De Luc and Wilcke.) Apart from lamenting these injustices, Brewster also disagreed with Harcourt's main argument, that Watt's hypothesis got the true composition of water wrong because 'hydrogen' and 'phlogiston' are not convertible terms. Watt, Brewster said, asserted that they were so convertible. Moreover, Cavendish and Black both considered Watt's hypothesis as a 'true' one. Thus Brewster did take Harcourt to task for the way in which he dealt with Watt's claim. The President's defence of Cavendish, though, Brewster entirely concurred with, and he regretted the imputations with which Arago had stained Cavendish's character.

Brewster's article was an ingenious compromise. It avoided the retrospective imposition of conceptual standards that Harcourt and Whewell relied upon. It maintained that the basic ideas of Cavendish and Watt were the same and acknowledged the circumstantial evidence for Watt's priority with the hypothesis. It used a methodological criterion to attribute discovery proper to Cavendish's sustained and precise experimental treatment.

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[101] Ibid., p. 495.
[102] Ibid., p. 496.
[103] Ibid.

No article by Brewster would be complete without a coda attacking the British government's inattention to the support of science and invention. The main thrust this time was the failure of the government to honour Watt, either in his lifetime with a peerage, or after his death with a *national* monument. In Brewster's view, the fact that the monuments that had been raised to Watt had relied upon private funding was typical of the voluntarism that plagued the progress of science in Britain.

The response to Brewster's article was ambivalent. Some people in both camps were pleasantly surprised or relieved. Henry Brougham believed that the author was 'quite right except in a few phrases'.[104] Muirhead felt that 'the mountain seems to have brought forth a mouse; but it is perhaps better that the birth should be a creature thus innocuous, than a Minerva armed'. He noted that Brewster was admiring of Watt and Arago, and, if rather too kind to Cavendish, he had at least attacked Harcourt. The water question, Muirhead felt, 'had perhaps been treated quite as well as could have been looked for' from Brewster. On the whole Muirhead considered that:

> We may thank our stars that the erring Knight has struggled through his slough of despond without more floundering; he has acted wisely in taking no notice of his great original lapsus, but the illustration of the joint-stock company, which he applies to Arago, might with a great deal more propriety be used with regard to himself.[105]

Watt Jr, however, considered Brewster to be persevering in error, and on the substantive point Muirhead agreed: 'I never can be persuaded, that the discovery of the *true* theory of the composition of water, is not the discovery of the composition of water; and with all respect for the chemists who seem to think otherwise, I think their doctrine, upon their own shewing, is ludicrous.'[106]

The Cavendish camp exhibited a similar ambivalence. Forbes considered the review fair on the whole. This rather surprised him 'considering the evidences of the authorship and the alliance with Lord Brougham'. Lord Burlington simply noted that the review 'seems to apportion the merit between Cavendish and Watt'.[107] However, the view was also circulating among Cavendish supporters that Brewster was wrong in allocating credit to Watt even for the theory:

> Brewster is quite wrong – that he has confounded an untenable hypothesis which Watt had early formed about the nature of water, (& which he formally gave up on learning Cavendish's exp^s^) with the opinion which he (Watt) formed on becoming acquainted thro' Priestley with Cavendish's Exp^s^ – the priority which Brewster claims for Watt is therefore priority in respect to an erroneous hypothesis …[108]

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104 Brougham to Napier, n.d. [January 1840], Napier Papers, British Library Add. MSS 34,621, vol. XI, ff. 5–6.

105 Muirhead to Watt Jr, 10 January 1840, Muirhead Papers, MS GEN 1354/462. The 'original lapsus' was Brewster's treatment of the water question in his *Edinburgh Encyclopaedia*.

106 Watt Jr to Muirhead, 10 January 1840, Muirhead Papers, MS GEN 1354/463 and Muirhead to Watt Jr, 24 January 1840, Muirhead Papers, MS GEN 1354/467.

107 J.D. Forbes to Whewell, 8 February 1840, (Copy), Forbes Papers, Letterbook III, pp. 50–53; Lord Burlington to W.V. Harcourt, 23 January 1840, *Harcourt Papers*, **14**, pp. 99–100.

108 Edward Sabine to Humphrey Lloyd, 13 January 1840, Royal Society of London, Terrestrial

While on the surface Brewster's compromise position in the water controversy was seen as such, representatives of both camps quickly moved to question the substantive points upon which he based that compromise.

Meanwhile Brewster's battles with the Cambridge men continued. As we have seen, in 1842 the *Edinburgh Review* published Brewster's review of Whewell's *Philosophy of the Inductive Sciences* in which he took issue directly with Whewell's conception of discovery, a conception that had by then been used by Harcourt to buttress his own case for Cavendish. Brewster's emphasis upon the importance of an experimental train of research in securing the discovery for Cavendish (and its absence in work securing only the theory for Watt) might have been reconciled with Herschel's view of discovery, with Forbes's, or even that of Harcourt himself. In practice, however, the wedge was being driven deeper between Brewster and the Cavendish camp. Brewster was outraged at the short shrift given to his own work by Whewell and at the treatment he received from Airy and Whewell over his optical papers submitted to the Royal Society of London. This at least was how Brewster represented the case to his friend Brougham:

> I am very anxious to have your Lordship's advice respecting some very harsh treatment which I have received from the Council of the Royal Society of London … The Council have lately rejected an original and valuable Paper, without assigning any reason, and refusing to mention the name of the Reporter on whose authority this was done. The truth is this Paper contains results and views hostile to the Undulatory Theory which seems now to be the Creed of the Society, I believe Airy is the person who has reported on my Paper, & who has done this entirely from personal feelings … The Royal Society needs Reform, as it is in the hands of a Cambridge Faction hostile to all Scotchmen.[109]

As we will see in a moment, when Brewster ceased to sit on the fence on the water question he also implicitly linked his own fate at the hands of the Royal Society establishment of the 1840s with what he took to be Watt's fate at the hands of its counterpart in the 1780s.

Brewster changed his stance on the water question in an article published in the *North British Review* in 1846.[110] The immediate occasion for the review was the appearance, under Muirhead's editorship, of the Watt *Correspondence* on the water question. The review also encompassed Muirhead's translation of Arago's *Eloge*, Harcourt's published 'Address' to the British Association, Brougham's lives of Watt and Cavendish, and Harcourt's letter to Brougham on his lives of Black, Watt and Cavendish.

The overall structure of the argument is ingenious, if a trifle disingenuous. Brewster, who was writing anonymously, named Drs Brewster and Hope as among those whom Watt Jr consulted shortly after his father's death about the

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Magnetism Papers, Vol. 1, MS119, letter 84. The view expressed was that of Robert Brown as reported by Sabine.

109 David Brewster to Henry Brougham, 14 December 1841, Brougham Papers, 26,624. Brewster subsequently described this as 'a case of persecution, arising as I am convinced, from the conduct of Airy & probably of Whewell' (Brewster to Brougham, 23 December 1841, 26,625).

110 [David Brewster], 'Watt and Cavendish – Controversy respecting the composition of water', *North British Review*, **6**, 1846, 473–508.

7.3 Dr David Brewster

water question, 'but these gentlemen not having seen the correspondence and judging only from the previously published documents, did not entertain the same opinion of the case as Mr Corrie and Dr Henry'.[111] The point being made is that Brewster and Hope did not believe in Watt's priority at that stage. (The claim might be seen as disingenuous because in his *Edinburgh Encyclopaedia* in 1830 Brewster contradicted Watt Jr's account in the *Britannica* ostensibly on the basis

[111] Ibid., pp. 475–76.

of having made a detailed examination 'some years ago' of the original documents.[112]) In his 1846 article Brewster presented himself as having been only recently convinced by the evidence in the *Correspondence* which took the reader behind the published documents. On this basis, Brewster unambiguously awarded the discovery to Watt.

Brewster, again not entirely consistently, adopted several of the argumentative strategies of the Watt camp. Thus the review was littered with judicial allusions. The reader was advised that the 'rules of evidence' are the same 'in the forum of science as well as that of law and justice', and at various stages Brewster described evidence as admissible or not. Muirhead's statement assigning the discovery to Watt by analogy with Godson on patents was cited with approval. Brewster also appealed to 'the jury of our readers' and referred to the 'ultimate decision of the public' as if everyman was a qualified judge of the matter.[113] However, Brewster also made an argument from expertise:

> We have ever thought that it is only a scientific man that can judge aright in a controversy, and that it is only an original inquirer who has anticipated others in discovery, and been himself anticipated, who can deal justly and tenderly with the great questions which involve the reputation of a philosopher, and affect the glory of his country.

We can take this as a statement that only those already 'blooded' in the matter of priority disputes, as victors and as victims, should exercise any judicial function in relation to them. It is hard not to see again a specific barb directed at Whewell and his ilk. Sounding uncannily like Robert Merton, Brewster continued:

> Such a man has a personal interest in the honest adjudication of scientific disputes. The case which he tries may be his own … An alien in the republic of letters cannot administer its laws. A pleader without its vernacular tongue cannot cross-question its witnesses. Hence do we exclude ourselves and all our anonymous craft from the bench of judicial science, and we call upon the Faradays, the Berzeliuses, the Liebigs, and the Dumas to eject us and occupy our place.[114]

It appears here that Brewster disqualified himself, at least rhetorically, as a judge, let alone the common man. He deferred to chemical expertise. But he pointedly disqualified 'all our anonymous craft', including presumably the anonymous reviewer who described Watt's theory of 1783 as 'unprofitable and worthless' (this was George Peacock) in much the manner that Harcourt dismissed the theory as 'an erroneous speculation'. Whewell was also quoted at length and associated with the interloping and unqualified judiciary. Watt's friends, we are

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112 Brewster's words were: 'We are not able at present to refer to the original documents, but we had occasion some years ago, along with a distinguished chemist, to examine them with minute attention, and it was then our decided conviction, that the merit of the discovery of the composition of water belonged to Mr. Cavendish' (*Edinburgh Encyclopaedia*, vol. 18, at p. 786). Evidently, by 'original documents' Brewster could not have meant 'original published documents' since there would have been nothing preventing him consulting them 'at present'.

113 Brewster, 'Watt and Cavendish', pp. 474, 497, 503, 481.

114 Ibid., pp. 501–502.

told, 'can have no anxiety about the truth or worth of a theory [Watt's] which the members of the Royal Society received with high approbation, and which Black, and Robison, and Henry, and Berzelius, and Dumas have accepted as a great chemical truth'. Even Cavendish himself, Brewster claimed, stated that Watt's theory was the same as his own, 'with only an apparent difference'. Clinching his argument from chemical authority, Brewster noted that 'living chemists of high name, Professor Graham, of University College, for example', had shown Watt's theory to be '*exactly similar* to those entertained by the most distinguished philosophers of the present day'.[115]

There are two points to be noted here. The first is that by explicitly contrasting the views of chemical experts with those of Harcourt, Whewell and Peacock, Brewster challenged the chemical credentials of those three gentlemen. The readiness with which they *assumed* judicial functions, whether in the writing of scientific history or in the adjudication of current claims (including some of Brewster's own), was under attack here. The second point is that Brewster, in the manner of Watt Jr or Muirhead, marshalled chemical expertise behind the idea of the sameness of the theory of Watt and the conclusion of Cavendish. This meant that his arguments – he does make arguments despite having disqualified himself – are now very much the arguments from circumstantial evidence favoured by the Watt camp.

In the last three pages of his review, Brewster manages to link the institutional state of science in Britain, the treatment of Watt by the Royal Society in the 1780s and, implicitly, his own situation. Returning to the themes of his declinist writings of nearly twenty years earlier, Brewster attributed the problems to the reliance in Britain upon the voluntary principle in forming scientific institutions. He described the Royal Society as an 'unnatural union … a copartnery of men of station and men of genius, – a collection of atoms of such opposite and incongruous properties, that even the electric spark of royal favour cannot effect their combination'.[116] In countries where talent was drawn from whatever station and properly supported in pursuing research, science prospered much more effectively, Brewster claimed. In Britain the voluntary principle led to the domination of the Royal Society by metropolitan and Oxbridge élites and by those wealthy gentlemen who could afford the fees and leisure required to participate in its forums. This meant, in Brewster's view, that the function of the organization 'must be performed in committees of various shades of capacity and knowledge'.

> The clashing interests of universities, castes, and professions, are all more or less represented and fostered in these judicial enclaves: But the provincial philosopher has no representation there, and whether he be a competitor for medals or fame, he will have little chance of success against an university or a metropolitan rival. And even if he is ambitious only of a niche for his discoveries in the Philosophical Transactions, or desires a testimony to the priority of his labours, he will succeed in neither, if some influential leader in the society, or some upstart member of a committee has been pursuing the same

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115 Ibid., p. 503. Roping in Thomas Graham in this way was a marvellous piece of mischief since he was at this time president of the Cavendish Society and of the Chemical Society of London. See Chapter 9, note 36.

116 Ibid., p. 506.

> train of research … From these observations, our readers will understand how the claims of Mr. Watt must have fared in a body thus constituted, and thus managed.[117]

So it was that Brewster, like his fellow Gentlemen of Science, but in a way quite contrary to them, linked the substance of the water controversy to arguments about the basic character of science in Britain.[118] Brewster now simply abandoned the convenient metaphysics of 'theory' versus 'discovery' that earlier seemed to provide him with a way of reconciling his convictions regarding the water question with his loyalty to his old friend Watt. He now adopted a quite different model of discovery, that of 'synonymity and priority', in his embrace of 'outsider' politics and the campaign against 'judicial enclaves' in British science, past and present.

Conclusion

The Gentlemen of Science who rallied around Harcourt and the cause of Cavendish exercised enormous power in various ways within the British scientific community. One way was in setting the scientific research agenda. This was done directly, as we have seen, through control of funds awarded by the British Association for research projects, and more indirectly by the creation of ideas about exemplary work. We will see in Chapter 9 how the figure of Cavendish became important to these people in the sense of providing an exemplar of scientific research and in developing an ideology of the relationship between abstract research and industrial change. The Gentlemen of Science also played a prominent part in public science through their contributions to the reviews and their role in scientific publishing generally. We have already seen a further instance of this in the way that the Gentlemen of Science and their friends shaped the reputation of Arago.

The battles in the reviews, and in particular the multi-strung tension between Whewell and the other Cambridge men, on the one hand, and Brewster on the other, indicate how the water controversy was conducted amidst an ideological maelstrom. Brewster's dramatic conversion to the Watt camp and the way that he managed it (and effectively explained it) confirms this. We have noted that Brewster's conversion was accompanied by liberal use of legal language in discussion of the discovery question. He was adopting, at least in part, the approach taken by three fellow Scots who were, both literally and figuratively, the advocates of Watt.

[117] Ibid., p. 508. Though he does not name them, Brewster is apparently referring to the Royal Society dissensions of the 1780s here.

[118] John Christie has diagnosed Brewster's tendency in all his historical writing to make very explicit links with his own obsessions. Writing about Brewster's *Memoirs of Sir Isaac Newton*, Christie notes that Brewster 'impose[s] upon the chronological narrative a dynamic oscillating structure, the reader being switched back and forth between past and present … A kind of egotistic teleology governs the movement of this pendulum: frequently, Newtonian history culminated in contemporary Brewsterian action.' See J.R.R. Christie, 'Sir David Brewster as an historian of science', in Morrison-Low and Christie (eds), *'Martyr of Science': Sir David Brewster 1781–1868*, p. 55.

Chapter 8

The Advocates of Watt: Brougham, Jeffrey and Muirhead

The water controversy at various times assumed the nature of a legal contest, with cases being made for the prosecution and the defence of the claimed priority of Watt. This might simply have been so because Watt's camp included a number of men whose professional training was in the law. However, the matter went beyond that. A particular understanding of the nature of discovery, what we have called the synonymity and priority model, lends itself to a legal approach in that its adherents assume the identity of the discoveries and contest priority purely on the grounds of circumstance. The cognate issue of patents of invention was very much to the fore in the period that concerns us, and this approach to discovery involved the perception of numerous and important analogies between discovery 'rights' and patent rights. Brougham and Muirhead gave considerable attention to the patent issue. Although I group these characters, together with Jeffrey, as advocates of Watt, and find similarities in many of their conceptions and argumentative strategies, they are also clearly individuals with differing reasons for involvement in the water controversy and in debates about Watt's reputation more generally. I will deal with each of them in turn.

Brougham

In the autumn of 1834, as Arago was making plans to travel to Edinburgh, and the British Association 'managers' were putting the final touches to arrangements for the meeting in that city, the government in which Henry, Lord Brougham had served was in disarray. Brougham was at a turning point. Since 1830, as Lord Chancellor in Earl Grey's government, he had played a crucial role in the passage of the Reform Act of 1832 and made significant strides in his law reform programme. Now Grey had resigned and the press, which Brougham had used so expertly for so long, had turned against him. Brougham was criticized for his self-obsessed ramblings during his speech at the Edinburgh dinner in honour of Earl Grey (also attended by Arago) after the British Association meeting and for the 'promotional' tour of Scotland that he took before and after it.[1]

In the wake of all this controversy Brougham retreated to Paris at the end of the year and was present at the meeting of the Institut on 8 December 1834 at which

[1] Robert Stewart, *Henry Brougham 1778–1868. His Public Career*, 1985, pp. 316–19; Ronald K. Huch, *Henry, Lord Brougham. The Later Years 1830–1868. The 'Great Actor'*, 1993, pp. 116–31; Morrell and Thackray, *Gentlemen of Science: Early Years*, p. 137.

Arago delivered his *éloge* of Watt. Even as his whole political future lay in the balance, Brougham could find time for this. Breakfasting with Arago and Pentland the next morning, Brougham remarked on the peculiar failure of Great Britain to honour Watt in his lifetime. The former Lord Chancellor vowed that if he were to return to that office, as he expected to do, it would probably involve a creation of peers and he intended to ensure that those creations did not neglect the descendants of both Watt and Arkwright.[2]

Brougham was always a compulsive writer as well as speaker. Sidney Smith cruelly but wittily referred to Brougham's literary productions as always long, 'like the penis of a jackass'. Even at the height of parliamentary business and chancery proceedings Brougham had contrived to maintain broader literary output. Sometimes, reputedly, when 'hearing' cases in chancery he was hard at work on essays for the *Edinburgh Review*, indifferent to the strenuous pleadings of the lawyers appearing before him. With his new-found freedom after the fall of the Grey administration he launched into a number of projects, among them his investigations into the 'water question' and the biographical inquiries that emerged later in his *Lives of Men of Letters and Science*.

By the 1830s Brougham could not be counted as more than an observer of the scientific scene although, characteristically, this did not hinder authoritative pronouncements. In his youth he had, no doubt, a remarkable command of the science of his time. Even as a student in Edinburgh he debated expertly with the professors of the university. In the late 1790s, his optical researches were published in the *Philosophical Transactions* of the Royal Society of London and he was the leading figure in the Academy of Physics in Edinburgh, a student society at the university that enjoyed a brief but stellar existence from 1797 to 1800. The pattern and tenor of their activities spoke of an 'inductivist egalitarian view of science'.[3] They were critical of the Tory-dominated Royal Society of Edinburgh and saw their Academy as an antidote to the senior body. A number of the Academy members, including Brougham and Francis Jeffrey, went on to be involved with the *Edinburgh Review*, which took a similar oppositional stance.

Brougham's output via the *Edinburgh Review* always contained a strong measure of commentary on scientific subjects, as did the *Review* itself in the early years. Indeed, Brougham's first public statement on the issues involved in the water controversy was made in an article for the *Edinburgh* in 1803. This was a review of the *Lectures* of Joseph Black that had just been published under the editorship of John Robison. As we have seen, this was a key document, albeit one requiring careful interpretation, in the historiography of the 'Scottish School' of chemistry in the late eighteenth century. What did Brougham make of the *Lectures*?

There are two major points to notice about Brougham's contribution at this stage. First he delineated a method discourse that he attributed to Black, to Black's pupil, colleague and editor, Robison, and that he, Brougham, also endorsed.[4] Second, he

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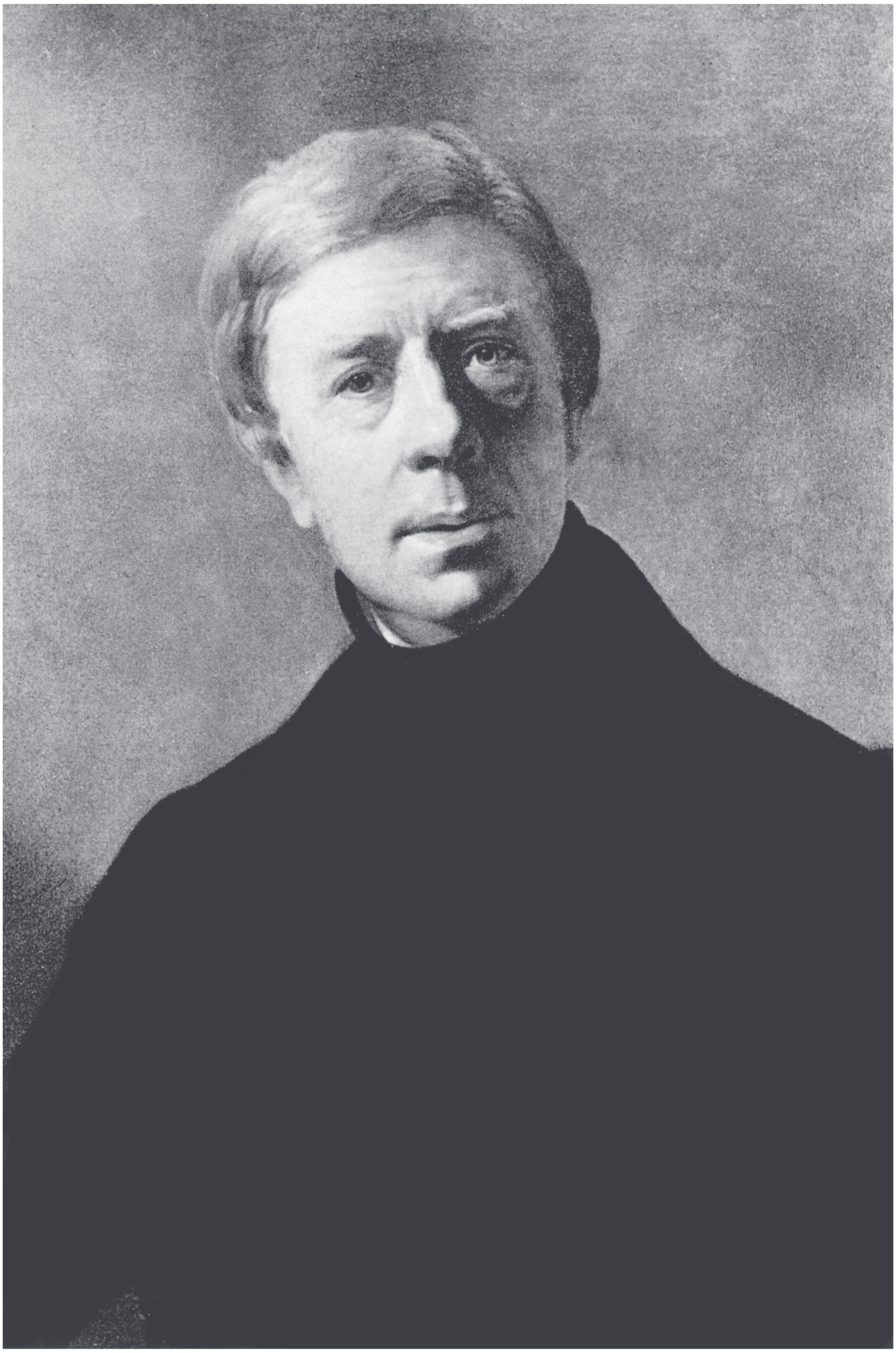
[2] J.B. Pentland to James Watt Jr, 12 December 1834, Watt Papers (Doldowlod), W/10.

[3] G.N. Cantor, 'The Academy of Physics at Edinburgh 1797–1800', *Social Studies of Science* **5**, 1975, 133.

[4] Cantor placed Brougham within a 'Scottish methodological tradition' strongly rejecting hypotheses involving unobservable entities (such as the ether) and explained Brougham's early tussles with



8.1 Henry, Lord Brougham

made some remarks about the significance of Black's discovery of latent heat and contrasted the mode of that discovery with the most common mode, one that applied, for instance, to the discovery of the composition of water.

The method discourse that Brougham endorsed might be characterized as 'cautious induction'. Black's own experimental work was treated as an example, and Brougham emphasized that Black was very cautious about theory and the erection of system. Black's teaching was also designed to be accessible and his points derived from clear, common, observable facts wherever possible so as to cater to his students. Black was quoted on the way in which he progressively integrated the work of Lavoisier into his lectures, refusing, on methodological and pedagogical grounds, to present Lavoisier's ideas as a system. Instead he interpolated particular facts and discoveries into his old lectures. Black made a virtue of the older material in the lectures:

> It will make the students acquainted with the chemistry of former years, which is far from being unworthy of the attention of a philosopher. Newton, Stahl, Margraaf, Cramer, Scheele, Bergmann, were geniuses not below the common level. But the person who learns chemistry by Lavoisier's scheme, may remain ignorant of all that was done by former chemists, and unable to read their excellent writings.[5]

Brougham himself endorsed this idea. He believed that the love of system had gone too far in France, in chemistry as in other areas, in seeking to obliterate the past. As he put it:

> The dogmatical spirit, indeed, with which the new nomenclature, and, in general, the new system, was promulgated, had a tendency to obliterate much very valuable information, contained in the writings of the elder chemists: and we conceive, that the present publication, if it serves no other end, would be highly important as a collection of things not to be met with in the works of the new school.[6]

However, sweeping away previous ideas was not just dangerous pedagogically and historically; it was also a threat to chemical progress. For, Brougham claimed, there was great danger in assuming that the new system could adequately account for all the facts. He praised Robison's notes to the *Lectures* for revealing where love of system has overcome careful induction from the facts. Brougham warned his readers against 'that implicit confidence in the universal truth of the antiphlogistic theory, which is derived from an unphilosophical carelessness about the facts, and a predetermination to learn the system synthetically'.[7]

Thomas Young over the wave versus corpuscular theories of light in terms of methodological differences. See G.N. Cantor, 'Henry Brougham and the Scottish methodological tradition', *Studies in the History and Philosophy of Science* **2**, 1971, 69–89.

[5] Joseph Black, *Lectures*, vol. 1, p. 549. The words are given by Robison as a direct quotation from a conversation with Black.

[6] Brougham, 'Lectures on the elements of chemistry', *Edinburgh Review*, **2**, 1803, 22.

[7] Ibid., 24. It should be noted that Robison puts these precise sentiments into Watt's mouth also during the discussions leading up to his editorship of the *Lectures*. See John Robison to George Black, 18 October 1800, Edinburgh University Library, Special Collections, Gen 874, VI, 11–12: 'Mr Watt

As a specific example, Brougham noted that Lavoisier's system did not cope inductively with light and heat: 'Lavoisier and his followers maintain, that the light and heat extricated during the combustion of inflammable bodies, come entirely from the oxygenous gas.' In maintaining this, Brougham claimed, they overlooked numerous chemical phenomena that seemed inconsistent with that idea.[8] Brougham was here pointing to a set of phenomena in the emission of heat and light in chemical processes that he believed the older chemists had treated differently and that Lavoisier and his followers had not adequately accounted for.

At this early date of 1803, then, we can see that the methodological stance taken by Brougham, consciously within the tradition of Black and Robison, gave only a limited welcome to the new system of chemistry. Brougham cautioned that much of value was in danger of being overlooked in the spirit of system. This left the door open for people to appreciate earlier ideas, including those about the composition of water, in their original integrity.

When he discussed the composition of water specifically, however, Brougham's approach was not that of an ardent advocate of Watt's priority claim. This is perhaps not surprising since Watt himself was scarcely making a fuss about the issue at that time. Brougham discussed the composition of water as an example to illustrate a more general point about discovery, that in most cases discoveries are the result of extensive work by a wide range of people. In the case of the composition of water, the doctrine was arrived at by 'many insensible gradations' from numerous contributed facts:

> some ingenious men, particularly Mr. Watt, reasoning from all these facts, concluded that this fluid is a compound of the two airs, deprived, by their union, of a considerable portion of their latent heat, the quantity (viz.) which is necessary for maintaining the elastic aeriform state. This idea was verified by the accurate experiment of Mr. Cavendish, in which the quantity of water formed was compared with the quantities of air burnt; and the French chemists added new proofs of the proposition by the analytical process. This chain of investigation is evidently so long, and of such slow formation, that we cannot, with any degree of correctness, appreciate the comparative merits of those who severally extended it; nor point out the particular link upon which the grand discovery hangs. And the same distribution of praise is strictly proper in almost all the other instances of successful physical research.[9]

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has given me another Notion of the subject [Black's lectures] from what I had allowed myself gradually to form. He considers it as a History of Chemistry for forty years, by one of its greatest Masters. Dr Black never attempted to give a System of Chemistry. He was unfriendly to all systems of an experimental Science … Dr Black pretended no such thing – but to make his hearers good Chemists – not able to talk about theories, but able to examine the Chemical properties of bodies, and to apply their knowledge … The philosophers, who want only refinements and new discoveries, will perhaps be disappointed – the System mongers will throw the book aside. But the public will be instructed … Such is the view that Mr Watt entertains of the Work … '. If this was indeed Watt's view, it reinforces the idea that after the first phase of the water controversy Watt retreated to the empirical world of chemical improvement. Perhaps there was a sense in which he had been there all along. To say so, however, would be to take sides!

8 Brougham, 'Lectures on the elements of chemistry', 24–25.

9 Ibid., 11–12. It should be noted that here Brougham gives the discovery as Watt conceived it and has Cavendish confirming it. Brougham thus rendered the doctrine of the composition of water

A point of contrast with this usual feature of discovery was, Brougham contended, Black's discovery of latent heat. Brougham claimed that in the case of latent heat, and of universal gravitation, the discoveries 'followed this *law of continuity*, in so slight a degree, that they may almost be allowed to form a case of exceptions to its operation'.[10] Black was thus put in Newton's company as not only a cautious but also a consummate inductive philosopher.

Brougham's earliest mention of the water question was thus non-committal as to priority, although it did give a prominent place to Watt. His discussion of Black in relation to the New Chemistry is, however, important in the light that it throws on the programmatic development of the science of chemistry at this time. Brougham was clearly prepared to see virtue in ideas and contributions that preceded, and in some ways lay outside, the new system of chemistry. We do not encounter Brougham engaged again with the water question, or with Watt's reputation more generally, until the 1820s. By this stage Brougham was functioning more conventionally as an advocate of Watt. He conducted his case less on matters of chemical substance, as it were, or methodological propriety, than on matters of literary circumstance and documentary evidence concerning it.

In the 1820s Brougham deployed Watt as a symbol in promoting the education of working men through the Mechanics' Institute movement. For example, in April 1825, on his installation as Lord Rector of the University of Glasgow, Brougham invoked the career of the great engineer. In doing so he perpetuated the notion of Watt as the pupil of Black in a way that would not have pleased Watt Jr but which made the connection between scientific education and the lower classes that Brougham sought. Noting that he spoke in an institution devoted to 'but a select portion of the community', Brougham nevertheless observed that

> from this classic ground have gone forth those whose genius, not their ancestry, enabled them; whose incredible merits have opened to all ranks the temple of science. I speak in that city where Black having once taught, and Watt learned, the grand experiment was afterwards made in one day, and with entire success, to demonstrate that the highest intellectual cultivation is perfectly compatible with the daily cares and toils of working men …

Brougham contended that the more widely knowledge was spread, the 'more Watts and Franklins will be enrolled among the lights of the world, in proportion as more thousands of the working classes, to which Franklin and Watt belonged, have their thoughts turned towards philosophy'.[11]

Watt was invoked in other contexts too. Brougham was a central figure in patent reform, seeking to improve the lot of the patentee. The patent law was discussed in his famous law reform speech of 1828, much to the delight of his old friend David

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differently from the way it was rendered in the Lavoisian system and effectively denied that the discovery of the composition of water had to be part and parcel of Lavoisier's New Chemistry.

[10] Ibid., 12.

[11] Henry Brougham, 'Inaugural discourse on being installed Lord Rector of the University of Glasgow, 6 April 1825', in *Speeches of Henry Brougham*, 4 vols, 1838, vol. 3, pp. 95–96. For further examples of the invocation of Watt, see Brougham's speeches to the Liverpool Mechanics' Institute and the Manchester Mechanics' Institution in July 1835, in *Speeches*, vol. 3, pp. 165–66 and 579–80.

Brewster, who was also agitating the issue.[12] Various Bills had been before Parliament in the 1820s and early 1830s without issue.[13] The failure of the 1833 Bill saw Brougham become more directly involved, and he subsequently introduced his own. Brougham's Bill was less ambitious than its predecessors. Its main provision was to employ the Judicial Committee of the Privy Council to decide on applications for the extension of patents. Such extensions previously required an Act of Parliament and that was an expensive process. Brougham also saw the Judicial Committee as dealing with disputes over priority of invention. In addition the Bill allowed changes to be made to specifications by disclaimer. It passed by a very narrow majority and was given Royal Assent on 10 September 1835. Some patent law reformers felt that the Bill did not go far enough. In particular, it did not deal with the cost of patents. Others believed, with Brougham, in the necessity of gradual reform, and welcomed the measure. It was after all the first successful piece of legislation on patents since the Statute of Monopolies. Brougham was also heavily involved in the Patent Law Amendment Act, which became law on 1 July 1852. This granted many of the provisions wished for by the invention interest, such as cheaper patents, a single patent for England, Scotland and Ireland, as well as changes designed to reduce the cost of litigation.

What interests us at this point is that Brougham, and others, used the example of James Watt's experience with patents to argue their point. The Select Committee of the House of Commons which sat in 1834 taking evidence on Brougham's Bill was shown, according to Brougham, that if Watt's statutory term in the patent had applied, he would have lost financially and that even when the patent was extended by Act of Parliament he spent many years out of pocket. The members of the Committee were advised that Watt would probably have fared better financially had he not taken out a patent in the first place.

> The Act which I introduced in 1835, grounded mainly upon that evidence [that is, the evidence relating to Watt], has removed some of the greatest defects in the law, and it has enabled, when coupled with the subsequent Act of last Session [an 1844 amendment], an inventor to obtain, at a very inconsiderable cost, his extension for any additional period, not exceeding the duration of the original patent.[14]

Brougham left no doubt that Watt was a scientific inventor who had deserved better protection from pirates and from mischievous assaults on patent specifications. Brougham took great interest in the process of patent specification. In 1828 he had personally drawn up the specification for J.B. Neilson's patent on the hot-blast technique. Neilson's case was regarded as having many parallels with that of Watt. We will see that J.P. Muirhead was also interested in the Neilson case, and inclined to see those same parallels.

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[12] Michael Lobban, 'Henry Brougham and law reform', *The English Historical Review*, **115**, 2000, 1184–215; Brewster to Brougham, 10 March 1828, Brougham Papers, University College London, 26,606; David Brewster, 'Decline of Science in England', *Quarterly Review*, **43**, 1830, 305–42.

[13] The following relies on H.I. Dutton, *The Patent System and Inventive Activity during the Industrial Revolution, 1750–1852*, 1984, pp. 42–51, 57–65.

[14] Henry, Lord Brougham, *Lives of Men of Letters and Science, who Flourished in the Time of George III*, 1845, pp. 378–79.

Both Brougham and Muirhead defended rights to invention and scientific discovery rights in the same kind of way. To them there was no material difference. First of all there was no moral concern, as there was in other quarters, about the contamination of scientific activity by commercial gain. The likes of Brougham and Muirhead could see no reason whatever why those who had sought and made commercial gain should not also partake of scientific fame.[15] Priority was to be defended in similar ways against those who would seek to exploit, on the one hand, gaps in the specification of a patented invention or, on the other, niceties of philosophical difference relating to discovery. For the Watt camp, the sly circumvention of a patent specification was of the same order as piracy of discovery by the fine metaphysical distinctions indulged in by the likes of Harcourt and Whewell as they sought to elevate Cavendish's conclusions above Watt's theory of the composition of water.

Brougham's direct involvement in the water controversy now needs to be outlined. We have seen that he had considered the water question as far back as 1803. He had been supplied with details concerning it by Watt Jr during the course of writing inscriptions for the Watt monuments. However, Brougham appears to have turned to the question seriously only after attending the delivery of Arago's *éloge* to the Institute in early December 1834. Brougham inserted a footnote concerning Watt's priority in his *Discourse of Natural Theology*, published in the following year, where he stated:

> Dr. Priestley drew no conclusion of the least value from his experiments [on the explosion of inflammable air]. But Mr. Watt, after thoroughly weighing them, by careful comparison with other facts, arrived at the opinion that they proved the composition of water. This may justly be said to have been the discovery of that great truth in chemical science. I have examined the evidence, and am convinced that he was the first discoverer, in point of time, although it is very possible that Mr. Cavendish may have arrived at the same truth from his own experiments, without any knowledge of Mr. Watt's earlier process of reasoning.[16]

From the mid-1830s, Brougham made a number of investigations into the water controversy. He located original papers in the Royal Society archives, discovering interpolations in Blagden's hand in Cavendish's original manuscript of his paper 'Experiments on Air'. These interpolations were significant, he believed, in resolving circumstantial questions about who knew what when, and, in particular, in revealing an effort by Blagden (and presumably Cavendish) to disguise the fact that Cavendish had reached no conclusions until he heard about Watt's theory. Brougham also gained access to the Cavendish manuscripts themselves, and Watt Jr had these inspected by W.T. Brande and Charles Hatchett. The chemists reported that they could find no evidence of clear conclusions in Cavendish's papers. Brougham finished work on a 'Historical Note' on the water question and it was agreed that it

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15 For debates on this issue see Iwan Rhys Morus, *Frankenstein's Children. Electricity, Exhibition and Experiment in Early-Nineteenth-Century London*, 1998, especially ch. 6.

16 Henry Brougham, *A Discourse of Natural Theology*, second edn, 1835, pp. 169–70. This note appeared as part of a discussion of induction in which Brougham used the analysis and synthesis of water as an example.

would be appended to Arago's *Eloge*. Watt Jr inspected the document and added a couple of footnotes that were printed in the final version. It was seen also by Muirhead.

Brougham's argument in the 'Historical Note' conformed substantially to the 'synonymity and priority' model of discovery. He assumed that the conclusions announced by Cavendish in his 1784 paper and Watt's theory were 'identical, with the single difference that Mr. Cavendish calls dephlogisticated air, water deprived of its phlogiston, and Mr. Watt says that water is composed of dephlogisticated air and phlogiston'.[17] The Note then relied upon the statement of a number of circumstantial facts leading to the conclusion that Watt had first put the conclusion in writing, that he did so without any conclusion being communicated from Cavendish, and that it was uncertain when Cavendish drew his conclusions. An interesting argument made by Brougham relied on an examination of Cavendish's publication practices. He claimed to show that Cavendish habitually published his experimental results and conclusions in the *Philosophical Transactions* quite soon after their prosecution. On the strength of this, Brougham suggested that Cavendish had no conclusions from his 1781 experiments at the time, otherwise he would have communicated them well before 1784.[18]

As of late 1839, when Arago's *Eloge* appeared (in French and in translation) with Brougham's 'Note' appended, Brougham had provided the most detailed and circumstantial case for Watt's priority. Although his later writings were to generate a good deal of reaction, they did not add substantially to his case. The account of Watt published in the *Lives* in 1845 reprinted the 'Historical Note'. The main text devoted less than three pages to the water question. It is notable for two things. The first was Brougham's denial that he had ever intended to insinuate 'a suspicion of Mr. Cavendish's having borrowed from Mr. Watt'.[19] The second notable comment addressed the unequal experimental activity of Watt and Cavendish:

> It must on no account be supposed that Watt cannot be considered as having discovered the composition of water, merely because he made no new experiments of particular moment, like Cavendish, to ascertain that capital point. No one refuses to Newton the discovery of gravitation … and yet he made not one of those observations upon which his theory rests … In like manner, Lavoisier, who discovered no gas, and made no original experiments of the least value in pneumatic chemistry, is universally admitted to have discovered the true theory of combustion and calcination, by reasoning on the facts which others had ascertained. Watt's happy inference from the facts discovered by Warltire and Priestley was just as much entitled, and for the same reasons, to be regarded as the discovery of the composition of water.[20]

[17] Henry Brougham, 'Historical Note', as reprinted in *Lives*, at p. 396. Brougham also portrayed *both* Watt and Cavendish as in the thrall of phlogiston theory. Two footnotes from Watt Jr extended the latitude given to identity by claiming that Watt's 1783 theory was essentially the same as even his earlier idea about water being convertible into air.

[18] Ibid., pp. 398–99. It was not claimed that Cavendish always published his work but rather that, when he did publish, he did so quite soon after performance of the investigation.

[19] Brougham, *Lives of Men of Letters and Science*, p. 381.

[20] Ibid., pp. 381–82.

This was, of course, a response to the arguments put forward by Harcourt, but also by Brewster, that Cavendish's experimental work gave him a claim of a type different in kind to that of Watt.[21] We will examine in Chapter 10 the detailed involvement of Brougham in the controversy during the mid-1840s.

For the moment we can say that Brougham was a powerful and important advocate of Watt. His involvement with the patent issue squared with a tendency to treat the water question as an analogous case to be argued on circumstantial evidence rather than on the minutiae of the specification. However, Brougham regarded himself as enough of a natural philosopher to be able to argue on chemical grounds also. When he did this he abandoned the preferred terms of engagement of Watt Jr and Muirhead. They believed that to enter the chemical arena was to concede too much to the chemical experts, like George Wilson, and the élite arbiters of science, like Whewell. Brougham's position was also complicated by his political relations with the Cavendish family and by his involvement with David Brewster in putting the case for a corpuscular theory of light against the dominant view supported by the leadership of the British Association and the Royal Society. In so far as it was possible for the Cavendish supporters to hold Brougham's scientific pretensions up to ridicule, Watt Jr's and Muirhead's decision about the grounds on which to fight their case was perhaps justified. Although they valued Brougham's support, they were also upset on more than one occasion by his cavalier approach to 'the facts'.

Muirhead

James Patrick Muirhead (1813–98) was born in Hamilton, Lanarkshire, the son of Lockhart Muirhead LLD, Regius Professor of Natural History at Glasgow University. Muirhead's mother was Anne Campbell and his maternal grandmother was a first cousin to James Watt. Muirhead was educated at Glasgow College and at Balliol College, Oxford. He was admitted advocate in Edinburgh in 1838 and spent eight years practising law there. He appears to have done work as a patent lawyer. In 1844 he married Katharine Elizabeth Boulton, second daughter of Matthew Robinson Boulton. Muirhead was thus doubly related to the Boulton & Watt concern.

Muirhead abandoned the law in 1846 and settled at Haseley Court in Oxfordshire in 1847. He had got to know James Watt Jr in the mid-1830s when he visited Aston Hall in the vacations during his studies at Oxford. Even at this stage Muirhead, who worked in the library at Aston Hall, seems to have fallen naturally into the role of a literary *aide-de-camp* to Watt Jr.[22] Watt Jr's plans of writing a memoir of his father

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[21] It should be noted that the artful linking of experimental results and conclusions of the kind that Harcourt and Brewster referred to might be judged differently than Brougham does. It could be claimed, for example, that although Lavoisier's experiments were rarely entirely novel, his *linking* of experiment and theory was. It might also be argued that the tradition of physical astronomy within which Newton worked had long since institutionalized a division of labour between observers and theorists. I make these points simply to indicate the interpretative flexibility available in such an argument.

[22] Muirhead to Watt Jr, 11 November 1834, 27 January 1835, Muirhead Papers, MS GEN 1354/265, 269.

were eventually transferred to Muirhead and this became his primary occupation for many years. Muirhead, too, participated at one remove in the filial spirit. When he was called to the bar Muirhead produced, as required, a Latin dissertation. He dedicated this to Watt Jr for his 'many kindnesses'. The dedication read: 'viri clarissimi filio claro', which Muirhead hoped that Watt Jr would like, because he knew well 'your feelings of veneration and love for your father's memory, and that it has always been deemed by you as a principal object of your life to preserve his fame in undiminished brightness … '.[23] In his capacity as chief historian to the filial project Muirhead produced first, in 1839, his translation of Arago's *Eloge* with original notes and appendix, then, successively, *The Correspondence of the late James Watt on his Discovery of the Theory of the Composition of Water* (1846), *The Origin and Progress of the Mechanical Inventions of James Watt* in three volumes (1854) and finally, *The Life of James Watt* (1858).

Muirhead's first major literary project with Watt Jr was his translation of Arago's *Eloge* of Watt. This came about in a rather odd way. Arago and Watt Jr had both agreed that Robert Jameson should publish a translation of the *Eloge* in the *Edinburgh New Philosophical Journal*, which Jameson edited. It appears that the initial plan was for Muirhead to help with the translation of the *Eloge* for Jameson's *Journal*. Rather quickly, however, Muirhead's translation became an independent venture. He expressed his aims as different from Professor Jameson's, given the rather limited readership of the *Journal*:

> first to gratify many individuals in the upper classes, who perhaps never see the Philosophical Journal; and secondly, to put the Memoir into the hands of the hundreds – I should rather say thousands, of intelligent mechanics, to whom it would be certainly very interesting and most probably very useful.[24]

In fact this bid for a more popular work directed not to experts but to the interested reader across the social scale, though consonant with the Watt camp's cause, was probably precipitated by a mix-up. Whilst Watt and Muirhead had understood that Jameson had agreed to Muirhead helping with the translation, Jameson himself professed no knowledge of the arrangement. He only learned of it, he claimed, when he had already received a full translation from another translator to whom he was now committed. Apart from undertaking the translation, Muirhead had added an extensive series of notes in elucidation of various points. Jameson saw the value of these and offered to add them to the other translation. Muirhead, however, resolved to publish separately as a distinct pamphlet. When Muirhead's project was well advanced, he was dismayed to learn that Jameson too was intent on publishing his translation of the *Eloge* as a separate pamphlet and not just in his *Journal*. Muirhead saw an advertisement for this work on 21 September 1839 and immediately went to see Jameson about it. Jameson and the publisher (Black) denied any knowledge of the venture while blaming each other! Muirhead was chastened and disillusioned about the motivations at work in the

[23] Muirhead to Watt Jr, 12 June 1838, Muirhead Papers, MS GEN 1354/280.

[24] Muirhead to Watt Jr, 31 July 1839, Muirhead Papers, MS GEN 1354/290. Muirhead also noted that his two audiences might require two forms of publication – a 'handsome 4to' and a 'practical 8vo'.

world of philosophical publishing.[25] Muirhead's translation duly appeared and was widely circulated by the translator and Watt Jr. In this way it became, as we have seen, one of the key documents in the water controversy.

Muirhead's next major project was the *Correspondence* on the water question itself. This had always been seen by Watt Jr and by Muirhead as the definitive answer to doubters and to the claims of Harcourt and his supporters. The evidence for Watt's priority lay in the sequence of letters between him and Priestley, De Luc and others that was eventually to be reproduced in the *Correspondence*. Watt Jr, though, was not over-eager to publish it. As we have seen, over the years he had shown it privately to many individuals and used it quite effectively as a behind-the-scenes lever of opinion. By the mid-1840s Watt Jr professed himself ready to release this major documentary source upon the world. He was exasperated by the persistent contrariness of Harcourt and his allies in the face of what Watt Jr took to be self-evident truths. His policy had been to let out only a little information at a time, but now he decided that the correspondence should become public to settle the matter once and for all. Unfortunately, by this stage Watt Jr's health was not good. His eyesight in particular was very poor. So he decided to entrust his faithful assistant Muirhead with the task of editing the correspondence. Watt Jr himself would supply a prefatory letter but Muirhead edited the correspondence and provided a substantial introduction to the work. That introduction became one of the most significant, and carefully crafted, statements of the case for Watt's priority.

How, then, did the editor Muirhead argue the case for Watt's claim to the discovery? To what extent did he engage with the arguments that Harcourt had tried to develop? To what extent did he retain a simple, empirical account of discovery strengthened by the 'revelatory' impact of the correspondence being introduced into the public domain?

Muirhead began by marshalling testimony to Watt's powers as a natural philosopher and, especially, as a chemist. Though self-taught, his natural powers gave him insight: 'How intently he watched the phenomena, how deeply he penetrated into the causes of chemical action … '.[26] Muirhead performed a delicate balancing act with regard to the chemical company that Watt should keep. Although anxious, in line with Watt Jr's sentiments, to show Watt as self-taught and not, at least formally, a student of Joseph Black, Muirhead nevertheless sought to assimilate Watt to Black rather than Priestley. According to Muirhead, Black was 'calm and reflective', conducting his experiments in a simple fashion. He was neat and accurate, and proceeded 'with all the force of exact demonstration'. Priestley, by contrast, was thrown to the wolves: though zealous and capable of great perseverance, Priestley was disordered and had 'an imperfect acquaintance with the true first

[25] Jameson had a long history of sharp dealing, including a feud with David Brewster in the early years of the *Edinburgh Philosophical Journal*. See W.H. Brock, 'Brewster as a scientific journalist', in A.D. Morrison-Low and J.R.R. Christie (eds), *'Martyr of Science': Sir David Brewster 1781–1868*, 1984, pp. 38–39. In 1826 Jameson became sole editor, for Constable, of what was then styled *The Edinburgh New Philosophical Journal*. Brewster promptly founded, with Blackwood, a new *Edinburgh Philosophical Journal* that survived until 1832 and was the main vehicle for his 'declinist' writings.

[26] J.P. Muirhead, 'Introductory Remarks', in Muirhead (ed.), *The Correspondence of the late James Watt on his Discovery of the Theory of the Composition of Water*, 1846, p. xx.

principles of science'. He proliferated experiments but had little sense of how to interpret them. Muirhead quoted Priestley's analogy between experimental philosophy and the beating of the ground during hunting as evidence of the crudity of Priestley's conceptions.[27]

Having consigned Priestley to the sphere of vagueness into which Harcourt and Peacock had sought to pitch Watt also, Muirhead now conjoined Watt and Black as the true Baconians:

> [W]e may perhaps question the propriety of applying language which conveys the idea of something vague and even fortuitous, to that system which Bacon first illustriously taught, and which Black and Watt so worthily exemplified; by which the present age has been guided to very many of the more remote and occult parts of nature, with the same certainty and safety, with which the compass has directed the course of navigation to the discovery of new regions of the globe.[28]

Thus for Muirhead, as for the other chief participants in the controversy, the alignment of their candidate discoverer with methodological propriety was of signal importance. As Brougham had done, Muirhead located methodological propriety in the likes of Joseph Black and his supposed method, one that was taken to involve cautious inductive procedure in the manner of Newton. Proceeding 'scientifically' was seen by both sides of the controversy as a crucial characteristic in a discoverer. Where they disagreed, of course, was in what it meant to so proceed.

The next step in arguing Watt's case was to frame the questions with which the discovery was concerned. Muirhead, as we saw in Chapter 2, rendered them thus:

> [W]ho first explained the real cause of the formation of the water, by drawing and stating the conclusion that water is composed of two gases, which unite in the process of their combustion, or explosion … Who was in point of fact the first to make public that theory, after having formed it altogether independently of the idea of others[?][29]

We noted in our discussion of the nature of discovery that framing the question in this way involved a number of presumptions about the criteria for 'discovery'. First, the conclusion that is to count as a discovery *must not only be drawn first, but it must be stated first*, by which is meant the making public of the conclusion. On this criterion, even if Cavendish had drawn the conclusion at the time of his 1781 experiments and even if he had stated the conclusion in his laboratory notes at the time, he would not qualify as the discoverer. Muirhead was framing the question in a way that played to his strong suit — whatever else might be said, Watt seemed to have pretty clear priority in drawing a conclusion *and* communicating it to others in writing.[30]

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[27] Ibid., p. xxvi.

[28] Ibid. On the varieties and uses of 'Baconianism' see Richard Yeo, 'An idol of the marketplace: Baconianism in nineteenth-century Britain, 1830–1917', *History of Science*, **23**, 1985, 251–98.

[29] Muirhead (ed.), *Correspondence*, p. xxxiv.

[30] This of course can be and was complicated by various factors. Thus it can be asked whether communication in a private letter counts; whether communication to the Royal Society counts or whether the communication being read at a meeting or being published in the *Philosophical Transactions*

Second, the conclusion itself should refer to the uniting of ‘two gases’ on their combustion to form water. The phrase ‘two gases’ is significant. Muirhead was playing down the issue of what those gases were.[31] Reference to the *production* of water in the experiments was not, on this framing, enough to qualify someone as a discoverer. Those who had previously observed a dewy deposit (for example, Priestley and Warltire) had assumed it to be a deposit of water previously present in the airs rather than created by their combustion. On the other hand, this phrase hedges the question of the identity of the two gases, a significant point given that one of the grounds of those arguing for Cavendish was that Watt was mistaken about what the gases were. The phrase also uses the terms ‘combustion’ and ‘explosion’ unproblematically, thereby sidelining the issue of the precise role of heat in the process. This, as we have seen, could be made into a major issue of difference between Cavendish’s and Watt’s conception of the composition of water. Muirhead resisted such a move and all was reduced to matters of circumstantial evidence.

Muirhead, following Brougham, found that Cavendish’s journal, as published in facsimile by Harcourt, had nothing to say about the origin of the water in his experiments. The whole contained nothing ‘inconsistent with the notion … of a mere mechanical deposit of the water’.[32] Cavendish’s writings and communications, Muirhead argued, did not provide conclusions from the experiments until the published paper of July 1784. That paper contained the following passage: ‘during the last summer [1783] also, a friend of mine gave some account of them [the experiments] to M. Lavoisier, as well as of the conclusion drawn from them, that dephlogisticated air is only water deprived of phlogiston’.[33] This passage, as Muirhead delightedly reported, was not contained in Cavendish’s paper as read to the Royal Society on 15 January 1784 but was added later, before publication, by Blagden. Muirhead concluded:

> There is thus no statement put on record by Mr Cavendish, so far as we have yet gone, of his conclusions having been either drawn by himself, or made known to a single human being, previous to the summer of 1783; while the only intimation to be derived from the printed papers in the Philosophical Transactions, of his having drawn his conclusions at even so early a period, is contained in the … passage, which was written by Blagden, interpolated after the paper had been read in January 1784, and then adopted by Cavendish.[34]

The lack of plausibility of Cavendish having drawn conclusions was confirmed, in Muirhead’s view, by the fact that his paper’s title, ‘Experiments on Air’, made no

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is the real point at which the ‘making public’ occurs. The difficulties here are precisely those involved in deciding whether Merton’s norm of communalism has been adhered to. In the end this is a matter for interpretation and negotiation. However a rule or norm is stated, a variety of actions can be made out to be in accordance with it. See Michael Mulkay, ‘Interpretation and the use of rules’, *Transactions of the New York Academy of Sciences, Series 2*, **9**, 1980, 111–25.

[31] Compare especially the approach of George Wilson discussed below.

[32] Muirhead (ed.), *Correspondence*, p. xxxvi.

[33] Cavendish, as quoted in ibid., p. xxxvii.

[34] Ibid., pp. xxxviii–xxxix.

mention of the key discovery that it supposedly reported. This contrasted with Watt's paper, which stated directly that it contained 'Thoughts on the Constituent Parts of Water, and of Dephlogisticated Air'.

The next phase of Muirhead's argument was devoted to showing that Cavendish, Blagden and Lavoisier were involved in purloining Watt's theory and trying to present it as their own. Then Muirhead hoed into Harcourt, regaling the reader with the latter's mistakes and inconsistencies. A similar demolition of George Peacock followed. Muirhead observed at one point (concerning the views of Dr Henry and the use made of them by Harcourt) that 'the question has become one of evidence much more than chemistry'.[35]

This last observation was true of Muirhead's whole argument. For him, at least publicly, there was no complexity involved in the chemical issues. The nature of the discovery itself was treated as very straightforward. The complexities lay in issues such as who knew what when, acts of commission and omission in communication, and consistency, or otherwise, of statement. Muirhead was impatient with those who indulged in nice distinctions. Harcourt was castigated for worrying unnecessarily and unproductively about what Watt understood by the term 'phlogiston'. Brewster was considered to be drawing distinctions too fine to worry about when he maintained that the priority of the *hypothesis* concerning the composition of water lay with Watt but the discovery of the *theory* of the composition of water lay with Cavendish.[36]

Muirhead, like Brougham and Jeffrey, came from a legal background.[37] All our legal writers were taken to task by their opponents for being unfamiliar with the requisite science. Both Brougham and Jeffrey privately relied on others, including George Wilson himself, to keep them on the scientific straight and narrow.[38] Jeffrey remarked to Wilson in the course of discussions of the water controversy in their correspondence: 'I know I am but a child in your mind in any question of chemistry and I have nothing but my poor Logic to combat your Science with. But (unluckily perhaps) I have taken up a strong impression that the question we have to consider is much more a question of logic than of science … '.[39] Where Muirhead, Brougham and Jeffrey did have an advantage, or so they felt, was in the argument of a case. This was why Muirhead sought to frame the controversy as a case involving an issue of evidence more than of chemistry.[40] James Watt Jr,

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35 Ibid., pp. lxxxiv–lxxxv, cxvii–cxviii.

36 Ibid., p. cxviii. Francis Jeffrey shared Muirhead's view of Brewster's fine distinctions. Jeffrey said of Brewster's distinction between awarding the *theory* to Watt and the *discovery* to Cavendish: 'The metaphysics of this principle of distribution seem to me as questionable as the application of the expressions is arbitrary and obscure.' (Jeffrey to George Wilson, 6 April [1847], Special Collections, University of Edinburgh Library, Dk.6.23/1/14–15.)

37 Wilson, *Life of the Honourable Henry Cavendish*, 1851, constantly refers, deliberately, I think, to 'the advocates of Watt'.

38 For Wilson's assistance to Brougham see, for example, Muirhead to Wilson, 18 March 1846, Muirhead Papers, MS GEN 1354/212, and for Wilson's help to Jeffrey see Jeffrey to Wilson, 11 September [1847], Dk.6.23/1/34 and n.d. [1847] Dk.6.23/1/38–39, Special Collections, Edinburgh University Library.

39 Jeffrey to Wilson, n.d. [1847] Dk.6.23/1/37, Special Collections, University of Edinburgh Library.

40 It must be acknowledged, though, that by the height of the controversy, Muirhead was probably as well versed in the chemistry of the controversy as anyone. He certainly considered himself to be.

in assigning the editorship of his father's correspondence on the water question to Muirhead, stated: 'As a question of evidence, this falls peculiarly within the sphere of your pursuits … '.[41] Muirhead concluded his most sustained account of the affair by drawing an analogy between the contest over priority of discovery and a patent case.

> Had Mr Watt's discovery of the theory of the composition of water been, like very many of his inventions, directly available for the increase of his own wealth, and, as such, protected by a patent, most certainly no case has been made out, on the part of Mr Cavendish, of such public use, or prior invention, as could have invalidated that patent.[42]

Muirhead then asked rhetorically whether the institution of science should be satisfied with any criteria in the award of priority of discovery less strict than those that apply in the award of a patent. Given the jealousy with which scientific fame is guarded and valued above pecuniary reward, the answer must, Muirhead contended, be no, and so a test of discovery modelled on the test of a patent he regarded as appropriate. So the claim was:

> First, that Mr. Watt formed the original idea in his own mind, and thus was A DISCOVERER of the true theory of the composition of water.
> Secondly, that being a discoverer, he was also THE FIRST PUBLISHER of that true theory.
> Thirdly, that being both a discoverer, and also the first publisher, he must therefore be held to be 'THE TRUE AND FIRST INVENTOR THEREOF'.[43]

An important feature of this rendition is that Watt is described simply as *a* discoverer. He need not be the *first* discoverer. Under the law of patents 'the prior discovery of an invention will not prevent another independent discoverer from obtaining a valid patent if the earlier inventor kept the secret to himself, the law holding that he is the "true and first inventor" who first obtains a patent'.[44] This legal definition of the true inventor is paralleled by analogy with the true discoverer. If the true inventor is defined by the act of taking out a first patent by *a* discoverer, then, by analogy, the true discoverer is defined by the act of first publishing an idea arrived at independently, though not necessarily first.

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41 Muirhead (ed.), *Correspondence*, p. ii.

42 Ibid., p. cxxi.

43 Muirhead stated that he was adopting the form of Godson on patents, pp. 27–30. The work in question is Richard Godson, *A Practical Treatise on the Law of Patents for Inventions and of Copyright*, 1823, 2nd edn, 1840. There are clear resonances between the water controversy and the patent controversy of this period, not least because Watt figured in both. See Christine MacLeod, 'Concepts of invention and the patent controversy in Britain', in R. Fox (ed.), *Technological Change. Methods and Themes in the History of Technology*, pp. 137–53. Each controversy also drew upon and shaped wider debates on the philosophy of discovery. These resonances were there in the beginning also. Thus Watt's correspondence in 1783 reveals his near simultaneous concern about plagiarism of steam-engine designs by Parisians and plagiarism of his ideas on water by Cavendish. See James Watt to Joseph Black, 3 February 1783 and Watt to Black, 21 April 1783 in Robinson and McKie, *Partners in Science*, pp. 121–22, 124–27.

44 Article 'Patents', in *Encyclopaedia Britannica*, ninth edn, vol. 18, p. 355.

The link between the water question and concerns about patents was a very real one for Muirhead. Throughout the period when the water controversy was at its height, Muirhead seems to have closely followed the fate of James Neilson in the latter's defence of his patent on the hot-blast technique.

James Beaumont Neilson (1792–1865), who was Glasgow born, was an engineman in the coal industry until, in 1817, he was appointed foreman of the Glasgow Gasworks. He promoted a variety of improvements in gas manufacture but is best known for his development of the hot-blast technique in iron making. He had investigated the smelting of iron and in 1825 read a paper at the Glasgow Philosophical Society on his findings. In 1828 he was granted a patent on the process of heating the air blast between the engine and the furnace. The patent specification was actually drawn up by Lord Brougham, who had performed similar services for Boulton & Watt.[45] With difficulty (because conventional wisdom was that the colder the air the better), Neilson persuaded the Clyde Ironworks in Glasgow to try out the technique. It proved very successful. Widespread adoption of the technique followed. Neilson entered into partnership with Charles Macintosh and Colin Dunlop to exploit the patent and promote the process. The patent was widely challenged and became the focus of litigation, first in 1832. From 1839, over five years, 'some twenty actions were proceeding in Scotland, and several in England. Three juries sat upon the subject at different times, and on three occasions appeals were carried to the House of Lords.'[46] In the Lords, Brougham participated, unperturbed, in deliberations on the validity of the patent that he had drawn up.[47]

During the height of the water controversy, the Neilson case was one of the key patent disputes. On 1 February 1845 Muirhead attended a victory dinner given for Neilson by his patent partners. Held in the Council Hall, Glasgow, the dinner was attended by almost one hundred, including 'all the most intelligent engineers, and nearly all the great ironmasters, of the neighbourhood, or, indeed, of the country'. Muirhead had been asked to give a toast to the memory of Watt, Arkwright and Murdock. He felt that someone not a relative of Watt might do this better, but agreed, on the understanding that he would not be required to eulogize at length. He need not have worried:

> the constant tribute of praise which was paid in the early part of the evening to your Father's memory, and the rapturous applause with which allusions to his discoveries

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[45] Archibald and Nan L. Clow, *The Chemical Revolution. A Contribution to Social Technology*, 1992 (originally published 1952), p. 355. Neilson must have seemed to be the ideal type of the adult-educated working man putting his knowledge to important use. Neilson had studied at the Andersonian Institution, whose founder, Dr George Birkbeck, was instrumental with Brougham in launching the Mechanics' Institute movement in the mid-1820s. In fact Neilson was instrumental in inaugurating the Glasgow Mechanics' Institution. He gave an address at the opening of its new rooms in 1825 in which he used Watt as an example of what could be achieved. See Thomas B. Mackenzie, *Life of James Beaumont Neilson F.R.S.*, 1928, pp. 9–10.

[46] Samuel Smiles, *Industrial Biography: Iron Workers and Tool Makers*, 1863, pp. 158–59. See also R.D. Corrins, 'The great Hot-Blast affair', *Industrial Archaeology*, **7**, 1970, 233–63.

[47] See, for example, the report of Househill Iron Co. v. Neilson on appeal before the Lords, in P.A. Hayward, *Hayward's Patent Cases, 1600–1883*, vol. 4, 1987, pp. 532–79.

> were received, saved me the necessity of saying more of the merit which I most deeply felt than you will find in the Notes which I enclose.[48]

Enclosed was a copy of Muirhead's speech. In it he linked Neilson, his activities and his patent struggle with the case of Watt:

> reflecting on the achievements which are inseparably connected with that most Illustrious and most Beloved Name [Watt], it cannot, I think, fail to strike us how similar in many particulars has been the course, how kindred in some respects must have been the spirit, of James Neilson, and James Watt. Born in the same district of country; – the science of both cradled in the same City; – their merits recognised and fostered by patrons of similar eminence and similar benignity; – both in like manner observing, considering, and investigating the nature, – the marvellous nature, and properties of Heat; both with like felicity deducing consequences important and beneficial to Man; – with like perseverance elaborating and perfecting their respective inventions; – threatened alike with repeated illegal infringements of their just rights, and both alike, after an arduous contest, finally and triumphantly successful: – I know not what is wanting to the truthfulness and completeness of the parallel …[49]

There can be little doubt from this that Muirhead considered that the *legal* defence of Neilson and the *historical* defence of Watt were complementary. According to his account, both inventors based their insights upon rigorous scientific investigation and not upon ingenuity alone. Both were investigators of nature, especially the nature of heat. Their claims to their discoveries were grounded in the same way.

Later, in his *Life of Watt* (1858), Muirhead was to remark on the way in which Watt's involvement in the patent contests 'sacrificed and consumed' the 'leisure and tranquility of a philosophic mind' and thus deprived humanity of 'further discoveries of refined beauty or extensive utility'.[50] Muirhead there also credited the law reforms introduced by Brougham with much improving the situation for contemporary investigators.

Jeffrey

Francis Jeffrey was less centrally involved in the water controversy than the other two advocates. His major public intervention was to write the article 'Watt or Cavendish?' for the *Edinburgh Review*, published in January 1848. The date is significant because some years earlier, Jeffrey, whose most recent contribution to the *Review* had appeared in 1840, had considered himself well enough retired to issue a four-volume *complete* compilation of his numerous contributions to that journal.[51] Jeffrey's first biographer, Lord Cockburn, had this to say:

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48 Muirhead to Watt Jr, 2 February 1845, Muirhead Papers, MS GEN 1354/775.

49 Enclosure in Muirhead to Watt Jr, 2 February 1845, MS GEN 1354/775.

50 J.P. Muirhead, *The Life of James Watt, with Selections from his Correspondence*, 1858, p. 403.

51 Francis Jeffrey, *Contributions to the Edinburgh Review in 4 volumes*, 1840. In the Preface, Jeffrey stated: 'I wrote the first article in the first number of the Review in October 1802: – and sent my last contribution to it, in October 1840!' The review of the 'water question' was, in fact to become his 200th contribution to the *Edinburgh Review*.

> During the autumn of this year [1847] he contributed his last article to the Review. It was the able and elaborate paper on the claims of Watt and Cavendish as the discoverers of the composition of water, which was published in January 1848. It would have been better perhaps if his final effort had been on a subject more congenial to his favourite tastes. But whether he shall turn out to be right, or to be wrong, in assigning the palm to his friend Watt, there can be no question as to the ability with which the evidence is discussed. He was always skilful in the art of arraying scientific proof.[52]

So there was a sense in which the judicious Jeffrey came out of retirement in order to adjudicate between Watt and Cavendish. However, this was not so strange as Cockburn found it. Jeffrey had a long prior involvement as an active participant in the 'reputational entrepreneurship' surrounding the figure of James Watt.[53]

The early days of the *Edinburgh Review* saw Jeffrey, as its editor, cooperate on at least one occasion with Watt Jr and others in a piece of reputational manipulation. It is sufficient here to recall that, according to Watt Jr, the *Review* was used in 1809, with Jeffrey's active cooperation, to publish a response to a group challenging Watt's engine improvements and hostile to the Soho engineering establishment.[54] A decade later, Jeffrey penned the obituary of Watt in *The Scotsman*, a much-quoted piece that most agreed did great justice to the departed engineer in both content and style. Watt Jr, in particular, admired the account and, when he wrote the brief sketch of his father's life for the *Supplement* to the *Encyclopaedia Britannica* in the mid-1820s, he agreed that Jeffrey's obituary should be appended to it. Watt Jr's diffidence as an author was matched by his admiration of Jeffrey's masterpiece.[55] This early association with the filial project helped to ensure Jeffrey's continued involvement in it. He was consulted extensively on the inscriptions for monuments to Watt that Brougham worked on, and took most of the credit for.[56] Watt Jr and Muirhead turned to Jeffrey, it seems, whenever they produced a new contribution to the literature.[57] Jeffrey was admired in part as a consummate stylist, but he was also greatly valued because, as Cockburn put it, he was so 'skilful in the art of arraying scientific proof'. Jeffrey was, after all, a judge, and, unlike his old friend Brougham, was also judicious. Whether handling evidence on paper, or people in conflict, Jeffrey was a polished performer. His skills as an editor, literary stylist, judge and

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52 Lord Cockburn, *Life of Lord Jeffrey with a Selection from his Correspondence*, 1852, 2 vols, vol. 1, pp. 402–403.

53 On 'reputational entrepreneurship' see Gary Alan Fine, *Difficult Reputations. Collective Memories of the Evil, Inept, and Controversial*, 2001, pp. 12, 16.

54 Watt Jr referred to this as the response to the 'Olynthiad'. See Watt Jr to Muirhead, 15 December 1844, Muirhead Papers, MS GEN 1354/766. The article, ostensibly by Playfair (and so attributed since), was, according to this letter, written initially by Watt Jr in consultation with his father and Mr Ambrose Weston and then edited by Playfair. See discussion in Chapter 5, above.

55 See Watt Jr to Francis Jeffrey, 11 January 1824, Watt Correspondence C6/10, which also asks Jeffrey to look over and revise the article. This he did, according to Napier to Watt Jr, 4 February 1824, Watt Correspondence, C6/10.

56 See Jeffrey to Brougham, 28 April 1832 and 1 November 1832, Brougham Papers, 43,083 and 15751, ff. 181–92.

57 On Jeffrey's advice on Muirhead's notes to his translation of the *Eloge* see Muirhead to Watt Jr, 22 October 1839, Muirhead Papers, MS GEN 1354/380.

diplomat were placed at Watt Jr's disposal. Thus, he not only advised upon much of what Watt Jr and Muirhead wrote, but he also assisted in negotiating the complex world of the *Reviews*. He helped them to manage people, most notably his old friend Brougham, who frequently required containment. As we have seen, in the case of Brewster's 1840 review in the *Edinburgh*, Jeffrey was briefed by Muirhead and acted as a go-between with the editor, Napier, trying to put pressure on Brewster via Napier to shape a favourable outcome for the Watt camp.

Jeffrey's letter to Napier on this matter is a fine example of his mastery of what we would now call 'spin'. Having noted that there is to be an article on Arago's *Eloge* of Watt, Jeffrey continued

> the existing (or filial) Watt is in a great pucker and flurry lest you should take part against the paternal shade on the question as to the composition of water, and is most anxious to have that part of the subject carefully, and, in so far as possible, *favourably* handled. He says Brougham was anxious to do it, but that you had already entrusted the subject to another, and he fears that other may be Brewster, who has (it seems) in some measure prejudicated the question in his *Encyclopaedia*. Now I, without pretending to *know* the whole merits of the controversy, confess that I participate in those feelings, and am confident that you, both as a Scotchman and a friend of so many of Watt's friends, must also have a leaning in their favour, though you, no doubt, have a *judicial* function to perform, on which favour can have no influence. The short of the matter, however, is that I wish you, if you have no objection, to tell me *who* your reviewer is to be, and whether he is to be for or against Watt upon this question. If he is against him, I shall merely report to W. that you decline giving him any information, and that he may rely on justice being done; while, if he is in his favour, perhaps you would not object to my letting him know that you incline to think such will be the view of the matter. At all events, you may rely on my silence and discretion as to whatever you may please to communicate; and though I should rather like to relieve the fat man, I really take no very eager interest in the matter. From the slight review I have taken of the subject, I incline to think that Priestley has fully as good a title to the discovery as either Watt or Cavendish.[58]

Watt Jr, from whom Jeffrey distanced himself in this letter by gently mocking him, had a very different impression of what was going on, as did Muirhead. From their perspective Jeffrey was their man, engaged in extracting as much information as possible from Napier about the reviewer and applying pressure for favourable treatment. It does seem that Jeffrey was feigning indifference in order to do this.

Jeffrey's major public contribution to the 'water question' in 1848 was thus, contra Cockburn, *not* on a topic distinct from his interests. It was in a cause with which Jeffrey had identified at least since the great engineer's death. Not only the article itself but also the immediate, private circumstances whereby Jeffrey came to write it, exemplify the judiciousness, and cunning, that made him such a valuable member of the Watt camp. Whilst there was a tradition of real and ritual obeisance to Jeffrey in early nineteenth-century Scottish literary circles,[59] Watt Jr's and

[58] Francis Jeffrey to Macvey Napier, 20 October 1839, in Macvey Napier Jr, *Selection from the Correspondence of the late Macvey Napier, Esq*., 1879, p. 305.

[59] On recapturing Jeffrey's early nineteenth-century reputation see James A. Greig, *Francis Jeffrey of the Edinburgh Review*, 1948, pp. 1–13.

Muirhead's estimation of him, and the value that they attached to his assistance, were quite genuine.

Jeffrey's essay of 1848 is interesting not least because it was produced at a time when there was considerable pressure on the Watt camp to supplement their empirical approach and address the chemical arguments of the other side. In doing this, chemical advice was sought and gained, as we have seen, from George Wilson. It had been intended that Wilson write the review in the *Edinburgh*, but the deal fell through and Jeffrey took on the task himself. It was agreed, however, that Jeffrey could make use of some of Wilson's material and we find the middle part of Jeffrey's article devoted to the 'charcoal argument'. This had been initiated by Harcourt and was to be a central focus of Wilson's *Life of the Hon^ble^ Henry Cavendish*. Without Wilson's aid Jeffrey could not have made this departure from his 'natural' mode of operation.

The article does, however, conform to the empirical, synonymity and priority, approach in most respects. It begins with a rehearsal of the key events of the 1780s and of beliefs about the claims of Watt and Cavendish as expressed in various subsequent publications. Jeffrey gradually assembled what he called 'testimonies'. He suggested that were the matter to be decided by authorities, it would be easy to resolve with the names of Henry, Brande, Davy, Brougham, Brewster, Arago, Dumas, and Berzelius on one side, and 'on the other, those only of Harcourt, Peacock and Whewell'.[60]

> But let the chemists say what they will, it is *not* a question of science or authority half so much as of Logic and Evidence; and if we did not think it one which might be fairly left to the judgment of educated men, with but a moderate reference to a few admitted facts and principles of chemistry, we should scarcely have presumed to judge it for ourselves, – and certainly should never have thought of submitting it to the judgment of our readers. As it is, however … upon the evidence now before us, we confidently expect to satisfy all who will take the trouble to follow us …[61]

So Jeffrey, in line with his legal brethren and others of the Watt camp, asserted the eligibility of everyman to judge such a question against the Cavendish camp's exclusion of popular judgement.

Jeffrey's article does read like a legal brief in many respects. However, on the parallels between a legal case and contests over scientific priority, Jeffrey added some important caveats: 'we can by no means adopt those narrow and jealous canons of evidence derived from the rigid maxims of law, or the precedents of cases of Patents, by which both M. Arago and Sir D. Brewster seem anxious to limit the inquiry'. In a court of law, Jeffrey explained, the object was not the truth as such but rather the 'import of the evidence that *is legally admissible*'. So in the law, for example, evidence from closely concerned parties may be inadmissible. In a case like the question between Watt and Cavendish, before a public tribunal 'no evidence is inadmissible'.[62] So the analogy between the water question and a legal question

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60 Francis Jeffrey, 'The discoverer of the composition of water; Watt or Cavendish?', *Edinburgh Review*, **87**, 1848, 86.

61 Ibid.

62 Ibid., 87

was limited. Even more limited, in Jeffrey's view (though clearly not in Muirhead's), was the analogy with the law of patents. This is because a patent is granted to the first to disclose an invention to the public and cannot then be invalidated by someone subsequently claiming prior invention. Where the glory of discovery is concerned, however, 'the palm of priority … may be justly awarded to one who has been forestalled in the publication'.[63] Jeffrey clearly regarded Muirhead's argument from analogy with patents as an insecure one. Whether it was or not depended upon what was regarded as publication. If the time of actual appearance in a journal was used (which might seem logical by parallel with the public disclosure of a patent specification), then Cavendish would have the advantage. Perhaps because his argument depended upon Watt's priority in less formal communication, namely his letters of April 1783, Jeffrey questioned the analogy.

Having demonstrated that Watt was 'an original and independent discoverer of the theory which he propounded in April 1783' because Watt claimed it then and Cavendish had no demonstrable prior claim, Jeffrey turned to a matter of science. What was Watt's theory and did it present a 'true explanation' of the nature and composition of water? On the face of it the answer was yes it did, since, according to Jeffrey, Watt's statement of the theory and Cavendish's were expressed in '*the very same terms*'; even Cavendish had said as much. However, Harcourt (and Wilson) had brought up the so-called 'charcoal argument'. This was directed at the meaning of 'inflammable air' as used by Cavendish and Watt. For their explanation to be seen as true, inflammable air had to be read as interchangeable with 'hydrogen'. This was certainly reasonable so far as Cavendish was concerned, because the inflammable air in his experiments had been prepared by the action of mineral acids upon zinc or iron. Priestley, however, whose experiments Watt relied upon, did not always make inflammable air in that way. Sometimes he made it by heating charcoal in closed vessels. Whilst Priestley made little distinction between the two types of inflammable air, we know that only one constituent, the hydrogen, can be converted in its entirety into water. Whilst the charcoal gas would contain a proportion of hydrogen produced from the water adhering adventitiously to the charcoal, it could not possibly all be converted to water on explosion with dephlogisticated, or atmospheric, air. It was, therefore, impossible for Watt to infer the correct theory from Priestley's experiments.

Jeffrey's response addressed the, illegitimate, assumption in Harcourt's charcoal argument that the only experiments from which Watt drew his conclusions were experiments done with charcoal gas. On the contrary, Jeffrey argued, Priestley did perform combustion experiments with hydrogen (inflammable air prepared from zinc or iron and dilute mineral acid) and Watt plainly based his conclusions on those. In this respect, then, they were no different from Cavendish's experiments.

On another point, Jeffrey conceded that Watt's discovery was 'somewhat obscured and embarrassed' by his adhesion to phlogistic doctrine. This was scarcely a unique position, however, since Black, Kirwan and Cavendish himself were also adherents of it. Harcourt's attempts to suggest that Cavendish was somehow more free of phlogistic doctrine than Watt, Jeffrey found unconvincing.

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[63] Ibid., 88.

After this long chemical excursion, Jeffrey returned to the business of circumstantial evidence of priority and independence of discovery. Much of this argument hung on whether Cavendish had drawn conclusions at the time of his experiments. Jeffrey made a range of ingenious arguments that turned on complex inferences from the statements of individuals. For example, the fact that Blagden began an account of the history of Cavendish's discovery in spring 1783 was significant, Jeffrey concluded, because Blagden, Cavendish's closest confidant, would surely have been told about it if the discovery were contemporaneous with the 1781 experiments, as Harcourt claimed. Here we see Jeffrey the barrister, cross-examining his historical witnesses in a virtuoso display of logical inference from evidence. When Jeffrey took on Harcourt's contention that the experiments of 1781 patently involved the conclusions and that Cavendish then drew them, we see Jeffrey's effective appeal to the psychology of belief, as if arguing before a jury:

> Now, we do not deny that there is, at first sight, something plausible and taking in this view of the matter; especially when addressed to a generation which has always been familiar with the conclusion, and with the universal assent of mankind in the sufficiency of the evidence referred to. Yet it requires but a moderate acquaintance with the actual history of the progress, even of the most obvious truths, and of the tenacity and vitality of prejudices and errors, to make us cease to wonder at the incredulity with which what is at last felt to be a demonstration, is often at first received.[64]

The plausibility of inertia in situations of potential radical discovery is argued in order to plead, contra Harcourt, that at a psychological level at least experiments cannot involve conclusions. Rather, people would believe that 'there must be a mistake somewhere, and the arrogant would scoff, and the thoughtful suspend their judgment accordingly'.[65] This is typical of Jeffrey's appeal to the judgement of plausibility of claims by 'everyman' against the 'expert' assertions of the likes of Harcourt.

Conclusion

In conclusion, we can say that the advocates of Watt brought their legal perspective to the water controversy. They favoured the model of discovery that dovetailed well with their legal approach and expertise. During the 1840s the question of the relationship between discovery and invention was open to negotiation and contest. Watt was a symbol of their legitimate proximity. Muirhead argued explicitly that the legal forms used to define the rights to an invention could also be applied to the matter of discovery. When they were, according to him, the dual claim of Watt to his steam-engine improvements and the discovery of the composition of water was secured. Jeffrey was as sure of those joint claims and also employed the empirical model of discovery. He was, however, involved in a long excursion over the charcoal argument, by way of defending the essential sameness of Cavendish's and Watt's

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[64] Ibid., 128.
[65] Ibid., 129.

views. This move into chemical argument was made possible by Wilson's advice and involvement, even though Wilson took the argument in other directions. Jeffrey left the Watt camp's favoured territory of argument, but he did so only to defend the contention that such chemical arguments were unnecessary and inconsequential and to return the question to a matter of logic and evidence. Though Jeffrey felt that his colleagues sometimes took the legal analogies of the case too literally, he certainly argued as if before a jury, and asserted the legitimacy of public judgement of the matter.

We turn now to the Cavendish camp to consider ways in which Cavendish, like Watt, served as a symbol, but of a different understanding of discovery, its meaning and significance.

Chapter 9

The Defence of Cavendish: Character, Precision and Discipline

> Cavendish, the millionaire, lives in a stable, eats nothing but mutton, and amuses himself – oh, solely for his private delectation – by anticipating the electrical discoveries of half a century. Glorious eccentrics![1]

Introduction

Was the Honourable Henry Cavendish honourable or not? This, as we have seen, was an important issue in the water controversy. The most contentious aspect of that controversy was the suggestion of dishonest dealings on Cavendish's part. Inevitably, then, the defence of Cavendish tended to be preoccupied with matters of character. However, even if the accusations of dishonesty had not been made, personal character would still have been a crucial issue because, for his defenders, it was intimately bound up with Cavendish's 'philosophical character', his precise mode of scientific work, and therefore was a basis for his claim to discovery.

Aldous Huxley placed the Honourable Henry Cavendish among the great English eccentrics. His peculiar behaviour and habits were widely remarked on in his lifetime and invariably mentioned by obituarists and early biographers. They remain a significant feature of historical folklore, no doubt rehearsed in school classrooms and university lecture halls to leaven discussions of the science that Cavendish pioneered.[2] Such accounts find significance in Cavendish's eccentricities mainly by way of illustrating the 'human aspect of science'. Probing a little more deeply, the connection is also often made between Cavendish's eccentricity and his genius and creativity. In this way, eccentricity becomes pathology, an organic condition of mind, and therefore plausibly linked to the creativity of that same mind. Taken to inhabit the nexus of genius and eccentricity, Cavendish was sometimes referred to as diseased. The chemist Thomas Thomson, for example, remarked in 1813 that Cavendish was 'shy and bashful to a degree bordering upon disease'.[3] Oliver Sacks

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[1] Aldous Huxley, *Crome Yellow. A Novel*, 1952, p. 72. First published 1921.

[2] A colleague has recorded his memory of receiving a variant anecdote, involving sausages rather than mutton, at school, and I too recall this. See David Oldroyd, 'Social and historical studies of science in the classroom?', *Social Studies of Science*, **20**, 1990, 751 and 756, n1. Educational websites relating to Cavendish and 'the Cavendish experiment' in particular often convey the anecdotes regarding Cavendish's character and must have greatly increased the prevalence and currency of these conceptions.

[3] Thomas Thomson, 'A biographical account of the Honourable Henry Cavendish', *Annals of Philosophy*, **1**, 1813, 6.

recently added Cavendish to his stable of oddly enlightening characters by tentatively diagnosing him as suffering from Asperger's Syndrome:

> Many of the characteristics that distinguished Cavendish are almost pathognomic of Asperger's syndrome: a striking literalness and directness of mind, extreme single-mindedness, a passion for calculation and quantitative exactitude, unconventional, stubbornly held ideas, and a disposition to use rigorously exact (rather than figurative) language – even in his rare nonscientific communication – coupled with a virtual incomprehension of social behaviors and human relationships.[4]

Sacks based his diagnosis on George Wilson's detailed portrayal of Cavendish's eccentricities.[5] He finds this evidence 'almost overwhelming', unlike the rather thin evidence for other recent claims that Einstein, Wittgenstein and Bartok were autistic. Sacks claims that George Wilson had 'a wondering admiration and sympathy for his subject'. Perhaps so, but Wilson also stated that he found Cavendish hard to like. In investigating the water question, even as Wilson became more and more convinced of Cavendish's claim to priority, he liked Watt more and Cavendish less. Wilson was an intensely religious, but also a highly sociable and fun-loving, personality despite severe bodily infirmities and illness. He found Cavendish's cold, clear intelligence admirable in its results but also inhuman.

Jungnickel and McCormmach's impressive revisionist account of Cavendish's work and character was inspired in part by what they regarded as the serious imbalances and deficiencies of Wilson's *Life*. They were disappointed that Wilson devoted so much space to the water controversy in a way that, in their view, distorted the overall picture of the great natural philosopher. They also disagree with Wilson's diagnosis of Cavendish as a man lacking passion and they argue that Cavendish was in fact a passionate and a social man within the delimited bounds of the experimental life. To react only to Cavendish's surface demeanour is to profoundly misrepresent Cavendish, in their view.[6] Although I am significantly persuaded by, and much indebted to, this work in various ways, my perspective is very different. I am concerned not so much to establish the reality of Cavendish's character as to deal with perceptions of it, and ideological uses of those perceptions in the nineteenth century. In seeking to understand the ideological uses of depictions of Cavendish's character I am pursuing a case employed in different ways by Robert K. Merton and by Steven Shapin.

In his work on the normative system of science, Merton frequently invoked the case of Cavendish.[7] He did so in two main connections: ambivalence about secrecy in science and the conduct of priority disputes. In Merton's account the stories

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[4] Oliver Sacks, 'Henry Cavendish: An early case of Asperger's syndrome?', *Neurology*, **57**, October 2001, 1347. Not surprisingly, given its appeal to the trope of the eccentric scientist, Sacks's story was picked up in the media. See for example, Erica Goode, 'Was scientist more than just odd?', *The Sun-Herald* (Sydney), 14 October 2001, p. 37.

[5] George Wilson, *The Life of the Hon^{ble} Henry Cavendish*, 1851.

[6] Christa Jungnickel and Russell McCormmach, *Cavendish. The Experimental Life*, 1999, pp. 10–15.

[7] See Robert K. Merton, *The Sociology of Science. Theoretical and Empirical Investigations*, 1973, pp. 274, 288, 291, 357–58.

about Cavendish's reclusiveness and his failure to publish much of his work produce ambivalence among fellow scientists. Although Cavendish was very productive, his secretiveness and failure to publish violated the norm of communalism and must therefore attract disapproval from those who might have earlier profited from his work. Indeed, all people who have the optimal functioning of the institution of science as a value must be critical of Cavendish on that score. Thus, in Merton's work the anecdotes about Cavendish were linked to what he regarded as basic regulative features of the scientific community. It would be possible, though inappropriate in my view, to apply this perspective to Cavendish in the context of the water controversy.

Another use of the stock of Cavendish anecdotes, particularly those relating to his supposedly peculiar dietary habits, is made by Steven Shapin in exploring those cultural forms or tropes that link bodily asceticism with the production of scientific knowledge. Shapin is interested in the way in which notions of disembodied knowledge so important to conceptions of objectivity are reinforced by tales, norms and stipulations about the bodily habits of natural philosophers. Isaac Newton and Robert Boyle provide probably more, and tastier, stories, but from the late eighteenth century Cavendish became a very popular subject and remained so through the nineteenth and into the twentieth centuries.[8]

Merton and Shapin, in their different ways, remind us that it is important when studying the uses made of Cavendish's character in the water controversy to recall that wider traditions and tropes were being drawn upon. It is also vital, however, to remain alive to the *particular* circumstances in which invocations of character were made. In this way, variations in the significance attached to the trope and the springs of ambivalence about Cavendish's character are in some way accounted for. As we will see, accounts of Cavendish's character, its significance and its consequences for his science generally and for the water controversy specifically, all involved interpretative flexibility. Different sides in the dispute were able to make quite convincing but opposing interpretations.

A key moral issue with Cavendish's character – one that Huxley was clearly referring to when he remarked on Cavendish amusing himself with research for 'private delectation' – was his failure to publish much of his research. It became apparent as Cavendish's private manuscript papers were examined that he had kept a great many of his important research findings to himself. The full extent of this secrecy so far as the electrical researches were concerned was revealed with their publication in 1879, under the editorship of James Clerk Maxwell.[9] However, aspects of the treasure trove had been revealed earlier by various scientists who had had access to Cavendish's manuscripts.

The Cavendish family in the early nineteenth century were interested in having Henry Cavendish's papers published. The then Duke of Devonshire entrusted the papers to James Hudson in about 1830 with a view to their being prepared for

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8 Steven Shapin, 'The philosopher and the chicken: On the dietetics of disembodied knowledge', in S. Shapin and C. Lawrence (eds), *Science Incarnate. Historical Embodiments of Natural Knowledge*, 1998, p. 41.

9 James Clerk Maxwell (ed.), *The Electrical Researches* (1879), included as vol. 1 of J.C. Maxwell (ed.), *The Scientific Papers of the Honourable Henry Cavendish, F.R.S.*, 1921.

publication, but Hudson appears to have held them for ten years or more without doing much with them.[10] William Cavendish, Lord Burlington, had been educated at Cambridge and, as we have seen, he became the major point of contact between the 'Gentlemen of Science' and the Cavendish family. In the aftermath of the Birmingham 'Address' it was via Burlington that Harcourt sought access to the Cavendish papers. Harcourt wrote on 23 September 1839, enclosing a copy of *The Athenaeum* of 31 August that contained an account of his 'Address', and formally requested access to Henry Cavendish's papers. In reply, Burlington noted that he had discussed this with the duke and learned that the papers were with Hudson, who had now been requested to send them to Burlington without delay.[11]

Harcourt had obtained access by January 1840 because he reported then to James David Forbes that the papers were

> exceedingly curious, in respect to the vast range of subjects which his [Cavendish's] inquiries embraced from the theory of the laws of motion & abstract questions of mathematics down, thro' the laws of heat electricity magnetism, sound etc., to more accurate geological observations than I believe anyone knew to have been made at that time. In his elaborate, & I imagine, unpublished exp^{ts} on heat which go back as far as the earliest of Black, there seems to be much deserving a more attentive perusal than I have yet been able to give them.[12]

Of course, Harcourt's primary purpose was to use Cavendish's experimental records to reinforce the case for his priority in the water question. We have seen how Harcourt did this. What interests us now is that in the published version of his 'Address' to the British Association Harcourt had a good deal to say about Cavendish's character. This is not surprising, since the 'Address' was a reaction to Arago's *Eloge*, which contained, according to the Cavendish camp, a scandalous set of aspersions on that character.

In comparing Cavendish and Watt, Harcourt stated that 'one stands as high in the discovery of natural facts, as the other does in their useful application'. In assigning them in this way to separate spheres of endeavour Harcourt also contrasted their genius while claiming not to place one above the other: 'let us hold a just and even balance between genius that rises superior to the pressure of circumstances, and that which reaches to at least equal intellectual heights, unseduced by rank and riches'.[13] This was Harcourt's interpretation of Cavendish's obsessive, solitary researches. They were not to be regarded as exhibiting a weakness of character because they were not always communicated publicly, but rather taken as an indication of strength of character in one whose wealth and station could have diverted him from the search for truth. In this interpretation Harcourt echoed Davy[14] and, notably, Cuvier, who had stated in his *Eloge* of Cavendish delivered in 1811 that:

[10] James Hudson was Assistant Secretary of the Royal Society of London for many years.

[11] Harcourt to Burlington, 23 September 1839 and Burlington to Harcourt, 28 September 1839, printed in *Harcourt Papers*, vol. xiv, pp. 98–99.

[12] Harcourt to James David Forbes, 11 January 1840, Forbes Papers, Incoming Letters, 1840, item 2.

[13] William Vernon Harcourt, 'Address', in *Report of the Ninth Meeting of the British Association for the Advancement of Science; held at Birmingham in August 1839*, 1840, pp. 6–7.

[14] Davy stated in 1810: 'in estimating the character of Mr Cavendish … his grand stimulus to

> celui dont nous allons vous entretenir a eu le mérite bien plus rare, et probablement bien plus grand, de ne pas se laisser vaincre par ceux de la prospérité. Ni sa naissance qui lui ouvrait un chemin facile vers les honneurs, ni de grandes richesses qui vinrent subitement lui offrir l'appât de tous les plaisirs, ne purent le detourner de son but; il n'eut pas même en vue la gloire ou les distinctions; l'amour désintéressé de la vérité fut son unique mobile.[15]

Recounting his examination of the Cavendish manuscripts, Harcourt noted how Cavendish had 'travelled over the whole range of natural philosophy', producing much undisplayed knowledge that the manuscripts could now reveal. Leaving the mechanical, meteorological, magnetical and electrical works to others, Harcourt ventured to say a little about the gems found in his examination of the chemical and geological papers, mentioning in particular Cavendish's observations and recording of geological strata, his deduction of all the laws of the 'generation and destruction of heat' including numerous determinations of specific heat, his elucidation of the chemistry of arsenic ten years before the published experiments of Scheele, and his priority in distinguishing nitrogen from other forms of unrespirable and incombustible gases.[16] Thus, Harcourt found in the manuscripts much material of scientific as well as of historical interest. He reproduced and extracted some of this material, remarking that it was 'calculated to throw a strong light on the character of Cavendish, a character never very common and – least of all now – that of a man pursuing truth for its own sake, communicative of it to his friends, but caring nothing for public fame'.[17] Harcourt's lament about the scarcity of such persons in the present gives a clue to one aspect of Cavendish's symbolic importance for the 'Gentlemen of Science'. The latter were keen, even as they argued for the utility of science, to protect and promote non-utilitarian research. Cavendish could be held up as an example of purity in less complex times.[18]

Cavendish's failure to publish could therefore be interpreted in a variety of ways. Whilst it might be seen as reprehensible, and certainly was so seen by Arago, for example, it could also be seen more positively. Harcourt's account of Cavendish made a virtue of it and many may have been persuaded. Michael Faraday was one. Thanking Harcourt for a copy of his 'Address', Faraday remarked: 'What a contrast does Cavendish present to those whose craving is so

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exertion was evidently the love of truth and knowledge: unambitious, unassuming, it was often with difficulty that he was persuaded to bring forward his important discoveries … he was, as it were, fearful of the voice of Fame.'

15 G. Cuvier, 'Eloge historique de Henri Cavendish', in *Eloges Historiques*, third edn, 1874, pp. 201–21, at p. 201 (The *éloge* dates from 1811). A translation is available in Eduard Farber (ed.), *Great Chemists*, 1961, pp. 229–38, which renders this passage as follows: 'The man who is the subject of our present discourse had the rare and probably greater merit of not permitting himself to be overcome by the obstacles of prosperity. Neither the accident of his birth, which made fame and honour easily attainable, nor great riches, which offered him the temptation of pleasure, could turn him away from his goal. He did not aspire to glory or distinction; the disinterested love of truth was his motivating force.'

16 Harcourt, 'Address', pp. 30–32.

17 Harcourt to Lord Burlington, 21 October [1840], Devonshire Collection, Chatsworth, 230–0.

18 See Morrell and Thackray, *Gentlemen of Science: Early Years*, pp. 423–24.

great for fame that it leads them almost to the verge of honesty. With what dignity his character shines forth!'[19]

The issue of Cavendish's general character, and through it his 'philosophical character', assumed great importance in the water controversy, and not only because his honesty was attacked by Arago and Brougham and defended by Harcourt and Wilson. According to Cavendish's supporters, the great man's character, his indifference to fame and his thoroughness in particular, allowed the controversy to happen in the first place. Cavendish did not publish his 1781 experiments and conclusions at the time because he felt that more experiments were required before he could make a definitive statement. Thus his thoroughness before being willing to publish created the gap that allowed Watt's claim room and *prima facie* plausibility. However, Cavendish's thoroughness and caution also meant that his work was, as Davy put it, 'finished', and did not have to be repeated, retraced or recalled. The argument was that it was better to have such superbly executed and secure work in due time than rushed incomplete contributions as they were first glimpsed. The comparison with the style of Priestley was an obvious one.

Cavendish's supporters also argued that his character made it impossible that he could have engaged in the duplicity of which Arago and others accused him. George Wilson in the *British Quarterly Review* in 1845 put it this way:

> Of all her illustrious philosophers, he was, without exception, the very last in reference to whom it was possible to believe that the accusation [of fraud] could be true. A man to whom applause had ever been hateful, and who had systematically avoided and declined the honours which his countrymen would willingly have conferred upon him, was not likely suddenly, and on a single occasion, to grow covetous of distinction, and to seek to gain it by fraud.[20]

The contrast between the approach of the Watt camp and that of Cavendish's supporters is that the former claimed to make no assumptions about Cavendish's character but rather to infer what his character must have been from examination of documents and events. Character was, they argued, something to be derived from the evidence. For the Cavendish camp, on the other hand, character was taken as given and therefore as itself evidence that the accusations of dishonest dealing must be mistaken. For them, too, the scientific consequences of his character were largely positive. Although it meant that some work was not published, it also gave the work that was published its 'finished' quality because of the thoroughness that Cavendish's general character engendered in his 'philosophical character'. George Peacock noted this feature of the man when he stated that Cavendish had 'the most cautious habits of reasoning, and never committed himself to a conclusion which his experiments and observations did not appear fully to justify'.[21]

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[19] Michael Faraday to William Vernon Harcourt, 24 October 1840, printed in Frank A.J.L. James (ed.), *The Correspondence of Michael Faraday. Volume 2*, 1993, p. 699.

[20] George Wilson, 'Lord Brougham's Men of Letters and Science', *British Quarterly Review*, **2**, 1845, 249.

[21] George Peacock, 'Arago and Brougham on Black, Cavendish, Priestley and Watt', *Quarterly Review*, **77**, 1845, 114.

By the time Sir Joseph Larmor wrote a Preface to his 1921 revision of Maxwell's edition of the *Electrical Researches*, Cavendish's work could be portrayed in an intriguing way:

> Careless though Cavendish was of scientific reputation, intent on pressing on to new solitary achievement, to the neglect of publication, due as it would seem as much to the habit of continual postponement of final preparations for the press as to the fascination of exercising his powers of discovery – and even, as it has proved, as a consequence of his recluse and self-centred life – there are perhaps few investigators of the first rank of whose work and aims and procedure we have now more complete knowledge than of his.[22]

Were we to conclude, perhaps, that Cavendish's work was successfully communicated precisely *because* he had been so tardy and inefficient in doing so in his lifetime?!

The Cavendish Experiment

For his supporters and admirers in the 1830s and beyond, Cavendish was not just a historical figure but a person of contemporary scientific reckoning and example. The Cambridge connection was important. After Newton, Cavendish came to be seen as that university's favourite scientific son. Like Newton, whose reputation was also a busy construction site at this time,[23] Cavendish became a powerful symbol. Beyond this, his work provided an exemplar of procedure and reasoning via experiment and mathematical representation. His legendary precision was something against which the best scientists of the age sought to test themselves. This was particularly the case with regard to the attempt to replicate Cavendish's measurement of the density of the earth. A number of individuals among the 'Gentlemen of Science' devoted much time to this. There was also a great deal of symbolic capital invested in it as exemplary. Successful replication would ratify and assist the major thrust of work in 'physique du globe' in Britain that was sponsored by the Gentlemen of Science at this time. Equally, aspersions upon the character and reputation of Cavendish presented a potential threat to this venture being pursued 'in his name'. This is one reason why the defence of Cavendish on the water question was a matter of some moment in Birmingham in 1839 and beyond.

The physics of the earth was a major focus of interest in British science from the 1820s. The conduct of pendulum experiments to determine the figure of the earth, the making of measurements of terrestrial magnetism and its variations, and the study of the tides were a focus for a confluence of interests among the Cambridge

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[22] Joseph Larmor, 'Preface' in James Clerk Maxwell (ed.), *The Scientific Papers of the Honourable Henry Cavendish, F.R.S. Volume 1. The Electrical Researches*, 1921, pp. vii–viii. These exculpations of Cavendish's secrecy are fine examples of the interpretative flexibility of the 'norm' of communalism in science. On this flexibility see Mulkay, 'Interpretation and the use of rules'.

[23] On Newton and constructions of his 'genius' at this time see Patricia Fara, *Newton: The Making of Genius*, 2002, pp. 202–30.

Network, scientific servicemen and mathematical practitioners.[24] Much of the lobbying of government by the leadership of the British Association, and a good deal of their expenditure of grant money for investigations, was related to these sorts of activities. The ventures were also characterized variously by the need to amass and control large quantities of data, to assess their reliability, and to take great pains in correcting results and adjusting them in the face of small but crucial sources of measurement, or experimental, error.

Within this context, Henry Cavendish's 'last experiment', his determination of the density of the earth, published in the *Philosophical Transactions* of the Royal Society in 1798, became one of great material and symbolic importance. Occupying fifty-seven pages in the *Transactions*, this paper is notable for its conception, its cautious prosecution and the insistence at all stages upon anticipating, and correcting for, potential errors in order to achieve the greatest accuracy possible.[25] Jungnickel and McCormmach make a crucial link between Cavendish's character and this experiment:

> Those traits that in his casual contact with people gave rise to anecdotes about his eccentricities were precisely the traits that in his scientific work made him extraordinary. To do science, Cavendish did not have to overcome his extreme diffidence but only to adapt it to science. The experiment on the density of the earth, *the* Cavendish experiment, is arguably not Cavendish's most important experiment, but if it is looked at for what it tells about the experimenter – as if it were a diary, which Cavendish did not keep, or a formal portrait, which he did not allow – it is the most revealing of his experiments.[26]

His most recent biographers thus consider the Cavendish experiment to determine the density of the earth as peculiarly revealing of the man's experimental life. As we have noted, their theme, contrary to those nineteenth-century and more recent accounts of Cavendish's 'coldness' and lack of passion, is that Cavendish led a passionate life of experiment, and even an active social life if one appreciates the importance to him of the small scientific circle with whom he interacted during the course of his experiments. Despite this, their perspective on Cavendish seems quite close to that of the 'Gentlemen of Science' in the 1830s and 1840s. Jungnickel and McCormmach believe that as well as working out his 'private destiny' through the experiment on the density of the earth, he also 'acted as the able representative of a general development in science' that being the 'drive for precision'. They mention that the new editor of the *Annalen der Physik*, L.W. Gilbert, in a 1799 foreword to

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[24] See David Philip Miller, 'The revival of the physical sciences in Britain, 1815–1840', *Osiris*, new series, **2**, 1986, 107–34; M.S. Reidy, 'The flux and reflux of science: the study of the tides and the organisation of early Victorian science', University of Minnesota, unpublished PhD thesis, 2000.

[25] For accessible accounts of the Cavendish experiment that also have some historical sensibility see: P.F. Titchmarsh, 'The Michell–Cavendish experiment', *The School Science Review*, **47**, March 1966, 320–30; B.E. Clotfelter, 'The Cavendish experiment as Cavendish knew it', *American Journal of Physics*, **55**, March 1987, 210–13; Isobel Falconer, 'Henry Cavendish: The man and the measurement', *Measurement Science and Technology*, **10**, June 1999, 470–77.

[26] Jungnickel and McCormmach, *Cavendish. The Experimental Life*, p. 453. See also Russell McCormmach, 'The last experiment of Henry Cavendish', in A.J. Kox and D.M. Siegel (eds), *No Truth Except in the Details. Essays in Honor of Martin J. Klein*, 1995, pp. 1–30.

the journal, called for the best work in physics in Germany to sit side by side in the journal with the best work from abroad. Gilbert cited Cavendish's experiment as an exemplar because of its wonderful exactness.[27] It was in the name of that development, its pursuit and institutionalization, that Cavendish became such a vital symbol for early Victorian science. This was especially so for those among scientific leaders ambitious to preside over education in the sciences and the increasingly organized prosecution of research.

In 1835, as the pursuit of physics of the earth gathered pace in Britain, the Council of the Royal Astronomical Society, on the suggestion of Augustus De Morgan, set up a committee to investigate the practicability of repeating the Cavendish experiment. The work was supported by a £500 grant from the government in 1837 to help defray costs. The person who took charge of the effort was Francis Baily, a retired banker, a founder of the Astronomical Society of London, and member of the Council of the Royal Society and of the inner circle of the British Association. Baily was the obvious person to engage in the kind of work involved in replicating the Cavendish experiment. He was the archetypal representative of thorough, precise and laborious work in science during this period. He devoted a great deal of time and energy to the reduction of astronomical observations and the collation of pendulum experiments undertaken to determine the figure of the earth. He was also centrally involved in determinations of the standard length. Baily has been accurately described as the 'perfected type' of the 'solid and sober, rather than brilliant' men who were so important to the Astronomical Society and, I would add, to ventures in the physics of the earth.[28]

The experiment almost defeated even Baily, who felt besieged by mysterious sources of error. A number of his friends were consulted on the experiment, including George Airy, the Astronomer Royal. In the end a suggestion from J.D. Forbes enabled Baily to obtain what he regarded as a satisfactory result.[29] The perceived importance of this result and the processes leading to it within the élite scientific community of the time are testified to in various ways. The Astronomical Society devoted the entire 14th volume of its *Memoirs* to the report of the experiments. Baily received the Society's Gold Medal in recognition of the work.[30]

We can see from this that the reputation of Cavendish was enhanced by the demonstrated difficulty of performing the Cavendish experiment to the degree of

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[27] Jungnickel and McCormmach, *Cavendish. The Experimental Life*, pp. 453, 456.

[28] See J.L.E. Dreyer and H.H. Turner, *History of the Royal Astronomical Society 1820–1920*, 1923, pp. 87–88; Timothy L. Alborn, 'The business of induction: Industry and genius in the language of British scientific reform, 1820–1840', *History of Science*, **34**, 1996, 91–121.

[29] See J.D. Forbes to George Airy, 18 April 1840; J.D. Forbes to Francis Baily, 3 August 1840, 2 March 1841 and 22 March 1843, all in Forbes Papers, Letterbook III, pp. 94–95, 117–19, 210–11, 504–505. As Harry Collins among others has taught us (*Changing Order*), the business of experimental replication is a complex one and the decision that an experiment has been successfully replicated is underdetermined by the experimental set-up and outcomes alone. The Cavendish experiment would make a good case study of such questions.

[30] Francis Baily, 'Experiments with the torsion rod for determining the mean density of the Earth', *Memoirs of the Royal Astronomical Society*, **14**, 1843, 1–120. It is notable that Baily actually defended Cavendish against some criticisms made by Dr Charles Hutton of the conduct of the original Cavendish experiment (pp. 88–92). Dreyer and Turner, *History of the Royal Astronomical Society*, pp. 91–92.

precision that he obtained. For Baily, and the other 'Gentlemen of Science' engaged in these ventures in the physical sciences, Cavendish was not just a historical figure but a ghostly leader and exemplary practitioner moving among them, a genuine scientific presence. Knowing this can help us to understand the sensitivity of that group to any attack upon Cavendish's standing within the wider community.

The Chemists and Cavendish

The fact that Cavendish had come to symbolize precision and best practice in natural philosophy was important not just to exponents of mathematical physics and *physique du globe* but also to members of the nascent chemical community in early Victorian Britain. It is at first puzzling that, with the notable exception of George Wilson, members of the developing chemical community of the 1840s did not play a significant part in public agitation over the water question. The reason for this may lie in the fact that the institutional development of chemistry was relatively late and problematic. Particularly vexed was the issue of the relations between 'pure', academic chemistry and practical chemical work. Those engaged in trying to build these delicate disciplinary structures in the 1840s were, I suggest, wary of public controversy, or perhaps too busy to engage in it, and were on the whole happy for the élite 'Gentlemen of Science' to carry the torch for Cavendish. At a time when there were strong centrifugal tendencies in the infant chemical community it would have been unwise for its leadership to engage too strenuously in a debate on the rival merits of Watt and Cavendish. It was however useful for the academic chemists, in maintaining their *de facto* leadership of the community, to identify with the disciplinary precision associated with Cavendish. The negotiation of curricula in particular, and defence of the idea that a generalist chemical curriculum could serve people destined for a wide range of occupations, including practical ones, acted out in microcosm the arguments over the utility of pure science. Cavendish was used by members of the chemical community to symbolize research pursued outside the trammels of practical exigency yet simultaneously of great potential and actual practical importance.

The 1840s was a key decade of institutional development in chemistry. The Chemical Society of London was established in 1841, as was the Pharmaceutical Society, institutionalizing the difference between the chemist 'proper' and the high street variety. The Royal College of Chemistry was founded in 1845. It has been convincingly argued that the composition and activities of the Chemical Society, and the curriculum and educational philosophy promoted in the Royal College of Chemistry, reflected the same institutional dilemma. This dilemma was how to achieve unity at some level among diverse academic, consulting and manufacturing chemists. Bud and Roberts depict the character of Victorian chemistry as an outcome of the relations between these three principal groups.[31] The second half of the

[31] Robert F. Bud and G.K. Roberts, *Science versus Practice: Chemistry in Victorian Britain*, 1984. See also G.K. Roberts, '"A Plea for Pure Science": The ascendency of academia in the making of the English Chemist, 1841–1914', in David Knight and Helge Kragh (eds), *The Making of the Chemist. The Social History of Chemistry in Europe, 1789–1914*, 1998, pp. 107–19.

nineteenth century saw institutional fission with the foundation of the Institute of Chemistry in 1877 and the Society of Chemical Industry in 1881, the former catering particularly to the professional qualifying concerns of chemical consultants and experts, the latter to the interests of industrial chemists and manufacturers. Yet despite this fission, the leadership of all these bodies retained a strong academic complexion. In effect, academic hegemony was maintained, moderating the centrifugal tendencies of the community.

Crucial to the establishment and maintenance of this hegemony was the articulation of the concepts of 'pure' and 'applied' science and their linkage with the notion of a liberal education in chemistry. 'Pure' science was research undertaken in pursuit of the facts of nature without concern for practical objectives. The knowledge gained thereby was to be regarded as valuable in itself but also of potential utility. 'Applied science' drew upon the stock of pure science in order to apply it in practical areas such as agriculture, industry, commerce or public health.[32] The educational philosophy supporting this – the idea of a liberal education in chemistry – maintained that a core academic education in chemistry could prepare budding chemists equally well, no matter what technical area they might subsequently enter. Disagreement and contention about whether this educational structure was indeed appropriate for the cadres of new technical personnel required in government institutions from the 1850s created the tensions leading to the split in the Chemical Society of London and the foundation of the Institute of Chemistry. However, in this case, as also in that of the Society of Chemical Industry a few years later, no serious challenge to the academic conception of appropriate education was established.

The Devonshire Commission of the early 1870s gave the academic conception a further boost with its recommendation for the establishment of science colleges in the provincial industrial areas. (That Commission was, of course, chaired by William Cavendish, Duke of Devonshire, who, as the Earl of Burlington, had represented the Cavendish family in the water controversy in the 1840s.) This was a vital step in the establishment of what became civic universities in centres such as Manchester, Liverpool, Newcastle, Birmingham and Leeds. Initially, however, students in these provincial colleges submitted to the University of London examinations. Further developments in technical education, especially in the 1890s, meant a fourfold increase in the number of students undertaking the chemistry examinations of the Department of Science and Art, from 5800 in 1876 to 24,000 in 1895. Within this expanding system, the training of technical specialists and of teachers alike partook primarily of the academic model.

James Donnelly has shown that student demand at the Royal College of Chemistry, at Owens College, Manchester, at University College, London and King's College, London was mainly motivated by interest in practical training. In the face of this there was mounted what Donnelly describes as

[32] G.K. Roberts, '"A Plea for Pure Science"', pp. 108–10. The categories of 'pure' and 'applied' science should not be thought of as watertight. No piece of chemical activity automatically identified itself as one type or the other. The deployment of these concepts should be treated as contingent boundary work of the sort discussed by Tom Gieryn, *Cultural Boundaries of Science: Credibility on the Line*, 1999.

> a campaign by the embryonic academic community of chemistry, and some of its 'lay' supporters, to exploit the utilitarian impetus for chemistry education and 'research' whilst retaining control and independence in relation to curricula and research … At the level of publicly-articulated ideology a battle was fought to uncouple the benefits of research and education from any immediate orientation to technical problems.[33]

Many of the leading chemists of the middle to later nineteenth century participated in this process: at University College London, Thomas Graham, Alexander W. Williamson and William Ramsay; at the Royal College of Chemistry, A.W. Hofmann and Edward Frankland; at Owens College, Manchester, Frankland, then Henry Enfield Roscoe. As students of the Royal College of Chemistry themselves moved into a variety of teaching positions, so the educational philosophy and chemical curriculum within which they had been trained was propagated.

So far as the water question in the 1840s was concerned, that cluster of chemists whose leadership intersected the Chemical Society of London, Section B of the British Association and the Cavendish Society found Cavendish an apposite figurehead as a pioneer of sophisticated, but cautious experimentation and quantification in chemistry and also as the most disinterested investigator imaginable. George Wilson, who had suggested that the chemical publishing society mooted by the London chemists be called the 'Boyle Society', took its naming instead after Cavendish as a sign that those same London chemists intended to support Cavendish in the water question.[34] This, however, was not a matter of individual predilections but rather a choice greatly influenced by the situation of the chemistry discipline at the time and, specifically, by the concern to unify its disparate groupings under the banner of 'pure science', 'applied science' and a hierarchical relationship between them.[35]

Wilson was amused that Thomas Graham was dragged publicly into the water question by David Brewster:

> I was highly delighted with the allusion to Professor Graham in the N.B. [*North British Review*] The poor professor will be astounded and seek in vain for the passage in all his works. He will be, moreover, terribly annoyed at being compelled in this strange way, to support Watt & take a side! He the cautious trimmer and President of the Cavendish! Unfortunately he is not a reader of reviews.[36]

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[33] James Donnelly, 'Industrial recruitment of chemistry students from English universities: A revaluation of its early importance', *The British Journal for the History of Science*, **24**, 1991, 8.

[34] George Wilson to J.P. Muirhead, 28 November 1846, as copied in Muirhead to Watt Jr, 30 November 1846, Muirhead Papers, MS GEN 1354/1055. See also W.H. Brock, 'The Cavendish Society's wonderful repository of chemistry', *Annals of Science*, **47**, 1990, 79.

[35] Among leading chemists the penchant for Cavendish seems to have prevailed regardless of other divisions within the chemical community. For example, it seems to have transcended the tension between those who emphasized the role of electricity in chemical phenomena and those who saw that concentration in British chemistry as a drag on the enterprise, and a reason for its failure to keep up with continental developments. See Bud and Roberts, *Science versus Practice*, pp. 40–45.

[36] Wilson to Muirhead, 27 February 1847, Muirhead Papers, MS GEN 1354/222. The article discussed is David Brewster, 'Watt and Cavendish', *North British Review*, **6**, 1846, 473–508. Brewster contended that Watt's theory had been shown by 'living chemists of high name, Professor Graham, of University College, for example … to be *exactly similar* to those entertained by the most distinguished philosophers

Wilson's amusement stemmed from Graham's known shyness of such conflicts. However, whatever personal dislike Graham may have had of public conflict, he had additional reasons for wanting to avoid being dragged publicly into the Watt versus Cavendish split and, indeed, for wishing that that conflict would go away. He and his colleagues sought a unifying symbolism, not a clash of cultures. Graham must have perceived a very real danger that the Watt cause might become a rallying point for those who rejected the ideological position being advanced by the chemical leadership with himself at their head.

We will see in some detail in the second attributional survey (Chapter 11) that the chemical texts produced by and for the burgeoning chemical community paid routine obeisance to Cavendish. Although leading members of the British chemical community tended to stay clear of public involvement in the water controversy, their texts provide some insight into their opinions. There was a tendency in certain quarters to grant at least a measure of credit to Watt, especially in the late 1840s and the 1850s, in the wake of the main period of controversy. However, the dominance of the pro-Cavendish position, and of attributions of the discovery to him, was quite clear.

In many ways George Wilson (1818–59) straddled the communities that supported Watt on the one hand and Cavendish on the other.[37] He was the most prominent, qualified British chemist to enter the controversy directly. Wilson was the son of an Argyllshire wine merchant. In 1832 he entered Edinburgh University as a medical student and in 1837 passed the examination of the Royal College of Surgeons in Edinburgh. During his studies he acquired a particular taste for chemistry. In 1838 he travelled to London (where his older brother Daniel was based) and worked as an unpaid assistant to Thomas Graham. In Graham's laboratory Wilson prepared his doctoral thesis on 'haloid salts of the electronegative metals'. Wilson failed to settle in London. He returned to Edinburgh and proceeded MD in June 1839. Importantly for our story, Wilson attended the British Association meeting in Birmingham that year, at which Harcourt delivered his defence of Cavendish. Wilson had been a close friend of the naturalist Edward Forbes since his student days and he was part of Forbes's circle in the Association. The members of this circle, though in many respects very critical of the organization's leadership, tended to share idealist views that would ally them with Harcourt and Whewell's camp.[38]

of the present day' (p. 503). The reference is almost certainly to work on the catalytic decomposition of water at high temperatures that was pursued in various ways by Graham, by William Grove and by George Wilson. Brewster indulged in a sleight of hand here in creating the impression that it is Watt's theory of 1783 that is receiving confirmation. The parallel, which Wilson also pointed to, was with Watt's *early ideas* about the convertibility of water into air by the action of heat. Wilson referred to Grove's work as an 'exact confirmation of Watt's early views'. See Wilson to Muirhead, 1 February 1847, Muirhead Papers, MS GEN 1354/218.

[37] On Wilson see: Jessie Aitken Wilson, *Memoir of George Wilson*, 1860; R.G.W. Anderson, '"What is Technology?": Education through museums in the mid-nineteenth century', *The British Journal for the History of Science*, **25**, 1992, 169–84; Charles D. Waterston, *Collections in Context: The Museum of the Royal Society of Edinburgh and the Inception of a National Museum of Scotland*, 1997.

[38] Wilson was a member of the 'Universal Brotherhood of Truth' (or Oineromathic Brotherhood), an informal association formed by a group of students at Edinburgh University and led by Edward Forbes. The members of this group maintained strong links during their later careers and displayed strong

Wilson abandoned medicine. He declined a minor chemical lectureship in London and began in 1840 to build himself a career as a lecturer in chemistry at various Edinburgh institutions. He worked hard and long hours to earn a living from his lecturing and writing. In 1845, for example, he reported writing ten lectures a week for four classes of students in the months before Christmas. As demand for his writings grew, he supplemented his income by publication. He advised a correspondent that he had undertaken the writing of a simple chemical text in order to pay the rent of his summer cottage.[39] The financial springs of his literary ventures should not be overlooked. Wilson's career reached its high point in his appointment in 1855 as Director of the Scottish Industrial Museum and, later in the same year, as Regius Professor of Technology in Edinburgh University. Wilson was best known for his work on colour blindness and for his *Life of Cavendish* (1851), which is, of course, the major source of our interest in him.

How did Wilson come to write this *Life*? It may be that his interest in Cavendish was sparked by his attendance at Harcourt's address to the British Association in 1839. Another connection was through Thomas Graham's circle. Graham was President of the chemical book publishing society established in 1846 and named the 'Cavendish Society', in whose series Wilson published the *Life of Cavendish*. W.H. Brock has explored the history of the Cavendish Society and believes its title 'was probably a deliberate anglophilic vindication of Henry Cavendish's work … '.[40] By the mid-1840s, when Wilson began a series of biographical sketches of leading chemists for the *British Quarterly Review*, he was also, apparently, in negotiation with the Cavendish Society for the opportunity to write the *Life* of the great philosopher. He told his close friend Daniel Macmillan, the publisher, tentatively about the commission in September and October 1846.[41] The opportunity may have come through George Day, the Secretary of the Cavendish Society, a friend of Wilson's from the London years in Graham's laboratory.

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idealist commitments. For Forbes and others these commitments manifested themselves in transcendentalist views of anatomy and natural history. It is likely that Wilson's chemical views were shaped by this philosophical outlook and that they were allied with Whewell's idealist treatment (including its application to chemical composition) of the philosophy of science in his *Philosophy of the Inductive Sciences*. See Philip F. Rehbock, *The Philosophical Naturalists. Themes in Early Nineteenth-Century British Biology*, 1983, pp. 68–91; Hannah Gay and John W. Gay, 'Brothers in science: Science and fraternal culture in nineteenth-century Britain', *History of Science*, **35**, 1997, 425–53 (428–31); E. Janet Browne, 'The making of the *Memoir* of Edward Forbes, F.R.S.', *Archives of Natural History*, **10**, 1981, 205–19. On the Forbes circle's tense relations with the BAAS leadership see Morrell and Thackray, *Gentlemen of Science: Early Years*, p. 138.

39 See George Wilson to Daniel Macmillan, 5 June 1848, Macmillan Papers, British Library, Add. MSS 55,089, ff. 101–102: 'At present I am writing a wee book for Robert Chambers on Chemistry, a thing to pay for Summer Lodgings, & warrant having no class.'

40 W.H. Brock, 'The Society for the Perpetuation of Gmelin: The Cavendish Society, 1846–1872', *Annals of Science*, **35**, 1978, 605. In line with my general argument, the anglophilia involved may have been secondary, not least given the prominence of Scottish-born chemists among the leadership of the Cavendish Society.

41 See Wilson to Daniel Macmillan, 17 September 1846 and 3 October 1846, Macmillan Papers, British Library, Add. MSS 55089, ff. 77–79, 80–83.

9.1 Professor George Wilson

Wilson was also connected, if more recently, with the Watt camp. He was on good terms with Muirhead after their meeting in February 1846. He helped with chemical advice to Muirhead and James Watt Jr and also on Lord Brougham's writings.[42] Muirhead and Lord Francis Jeffrey made strenuous attempts to recruit Wilson as a reviewer of Muirhead's book on the water controversy for the *Edinburgh*

[42] See the discussion of their relationship in Chapter 10.

Review. The review of the *Correspondence* was of course of great importance to the Watt camp and the process was carefully watched and engineered. Wilson's competence was respected and he would be a valuable person from whom to secure a positive review. This was because he was a graduate of Thomas Graham's group and therefore one of the 'New Chemists'. Also, as an Edinburgh chemical lecturer, Wilson was in a position to influence chemical opinion.[43] While Muirhead and Francis Jeffrey were keen to recruit Wilson, Watt Jr appears to have gone along rather reluctantly, not least because he doubted the wisdom of making the water question a chemical one.

Wilson did accept the commission and produced an article for the *Review*. But Francis Jeffrey, who had taken it upon himself to deal with this in the 'interregnum' between the death of Macvey Napier, the *Review*'s editor, and the appointment of his successor, considered that Wilson's effort failed to give Watt sufficient credit. Jeffrey tried to induce Wilson to make suitable additions, but Wilson seems to have refused any such compromise. Wilson eventually withdrew the article but was paid for it and subsequently advised Jeffrey on the latter's own version of the article.[44] Watt Jr felt that his reservations about 'the Edinburgh Lecturer on Chemistry' had been well founded. As he remarked to Brougham:

> After the numerous professions of impartiality made by the Edinburgh Lecturer on Chemistry, we could not have anticipated such conduct on his part, and his dogged obstinacy [in refusing to be persuaded of Watt's case] surpasses all belief … As to an intended use of it in any other ways he had best be cautious, as he may otherwise meet with worse usage than the dreaded displeasure of Harcourt, Whewell & the Dean of Ely [Peacock] could have caused.[45]

It seems most likely that Wilson's work on the piece that so annoyed Watt Jr did become the kernel of the later *Life of Cavendish*, perhaps partly explaining the narrow focus of the work. In his negotiations with Jeffrey and Muirhead, Wilson was happy enough to praise Watt in the sense of acknowledging that he had much merit in the affair. Wilson insisted, however, that Cavendish's claims to the discovery were firmly established and he totally resisted the implication by Arago and Brougham of dishonesty on Cavendish's part.[46] In fact Wilson authored a swingeing attack on Brougham in these terms in the *British Quarterly Review.* He was more cautious, however, about affronting others involved in the controversy. In declining at one stage to undertake the review of Muirhead for the *Edinburgh Review*, Wilson, who had just read Brewster's 'conversion' article in the *North British Review*, stated: 'The N.B. [*North British*] article will be highly gratifying to Arago, Dumas, & Lord Brougham:

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43 Although the review would have been anonymous, news of authorship frequently circulated quite freely and so any endorsement that Wilson might give to the Watt case would have been a real one.

44 This account is reconstructed from Francis Jeffrey's letters to Wilson and from the Muirhead/Wilson correspondence; see particularly Jeffrey to Wilson, 10 August 1847, Dk.6. 23/1/16–17 and Jeffrey to Wilson, n.d. [1847], Dk.6. 23/1/37, Special Collections, Edinburgh University Library. The review that was finally published was [Francis Jeffrey], 'The Discoverer of the Composition of Water; Watt or Cavendish?', *Edinburgh Review*, **87**, 1848, 67–137.

45 James Watt Jr to Henry Brougham, 17 September 1847, Brougham Papers, 29,218.

46 See Wilson to Muirhead, 18 December 1846, Muirhead Papers, MS GEN 1354/214.

and grievously vexatious to Harcourt & Whewell. It were best then to "let well alone" as I cannot afford like Sir David to make these last transgressors my mortal foes, and I am unable unfortunately to satisfy the first illustrious three.'[47] Here is clear evidence that the ideological stance and power of the 'Gentlemen of Science' continued to shape the controversy for subsequent key authors.

Wilson's career as a chemical lecturer in Edinburgh depended upon recruiting students from among those engaged in medical studies in the town. A significant number were former Cambridge men, whom Wilson described as keeping him on his toes.[48] Figures such as Whewell and Peacock would have exercised considerable influence over the choice of lecturers by Cambridge men studying in Edinburgh. Thus there were rather direct, career-related, reasons why Wilson could not afford to make foes of Whewell and Harcourt. Although Wilson's *Life of Cavendish* was not a 'party' production, it was inevitably shaped by the larger ideological battle.

We gain insight into Wilson's relations with Cambridge people from his correspondence with his old friend Daniel Macmillan, who was intimately involved in the Cambridge scene. Wilson on the one hand appeared very concerned about what the Cambridge people thought about him, especially Whewell and Peacock.[49] Yet he was also quite willing to criticize them, albeit privately. The *History of the Inductive Sciences* was found to be inaccurate. Wilson, having been informed by Macmillan of the views around Cambridge of Scottish universities and the talk that the productions of 'Scotchmen' were never very accurate, wrote a humourous reply worth quoting at length:

> In sober seriousness my good Daniel what tempted you to repeat these 'havers' about Scotch ignorance. Most true it is that Scotchmen are not 'very accurate'. Neither are Frenchmen, nor Germans, nor Dutchmen, nor Poles, nor Russians, nor Norwegians, nor Swedes, nor Persians, nor Chinese, nor Turks, nor Caffres, nor Esqimaux. Only Rome is infallible. Oxford perhaps which casts sheeps' eyes towards the Seven Hills may put in a claim that way also. But I never heard that Cambridge could.[50]

The scoffers in Cambridge need not go across the Tweed 'or even across the Cam' to find examples of not very accurate men. He proceeded to enumerate examples of mistakes in Cambridge mathematical texts. Then, writing of Whewell, he said:

> Dare one scoff at him? Can the Head of Trinity be a blunderer? Is it possible that he is a Scotchman? He must be, for he is "very inaccurate". His presumptious history of the

[47] George Wilson to J.P. Muirhead, 1 February 1847, Muirhead Papers, MS GEN 1354/218.

[48] Jessie Aitken Wilson, *Memoir of George Wilson MD, FRSE*, 1860, pp. 245, 254. Wilson considered himself in 1840 as 'pretty certain of getting the Cambridge men, one and all' in his lecture course. It is likely that by the time he was getting embroiled in the water controversy he had built a steady clientele from that source.

[49] See, for example, Wilson to Macmillan, 6 August 1847, British Library, Add. MSS 55,089, ff. 93–94, in which Wilson told Macmillan about his forthcoming *Edinburgh Review* article (which of course never actually came forth!) and asked to be told whatever Macmillan could learn about Whewell's and Peacock's reaction to it.

[50] Wilson to Macmillan, 11 April 1847, Macmillan Papers, British Library, Add. MSS 55,089, ff. 90–92.

> Inductive Sciences is wofully [*sic*] shamefully inaccurate. I do not say throughout, but on the subjects I know slightly, I pity the still slighter knowledge, & most pretentious ignorance of Master W.

Nor did Peacock escape:

> Can he be Scotch also? Depend upon it he is, for how else can his blundering be accounted for[?] About a year ago, I was consulted as to the merits of an Article on the Water Question, which appeared in the Quarterly Review. I did not know its Author, but I was compelled to acknowledge that though it advocated my views, it was full of most discreditable chemical inaccuracies. It has since been acknowledged by Dean Peacock, who poor man is very proud of it.[51]

This captures well the attitude of Wilson, the true chemical expert and 'Scotchman', to the ideologues of Cambridge. Yet he was careful, and wise, to express such views privately, and then only to close friends.

The arguments presented by Wilson on behalf of Cavendish were detailed, complex and thorough. In a systematic fashion Wilson examined not only the writings and actions of the major protagonists in the original controversy but also *every* argument mounted in the subsequent literature by Watt Jr, Muirhead, Brougham, Arago, Harcourt, Brewster, Jeffrey and others. My chief concern is with Wilson's approach to the issue of 'discovery'. What distinguished Wilson's work, and what also makes it extremely difficult to read, is that he went to great lengths to contextualize both the knowledge and actions of protagonists. For example, we learn much more from Wilson than from anyone else about the experiments of Cavendish, Priestley, Lavoisier and others. In particular, Wilson was very concerned to show what Cavendish and Priestley *really meant* by such key terms as 'phlogiston', 'inflammable air' and 'air'. In doing this he entered into their intellectual life-worlds in a way that is much in tune with modern empathic historiography.[52] Yet no sooner had Wilson done this than he engaged in presentist scientific judgements about whether the protagonists' knowledge was sound or not, and whether it could have been sound given what they *really* had physically in their apparatus.

In the same vein as Harcourt, but in much more detail, Wilson argued that Priestley's experiment, because he used charcoal in preparing his inflammable air, could not have produced the pure mixture of gases in the correct ratio to produce only water on combustion. Therefore, in so far as Watt's inferences were correct (that too is disputed but can be left aside for the moment), they *could not have been properly drawn* from Priestley's experiments. Thus, either we say that Watt drew

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51 Ibid.

52 A good example of Wilson's sometimes impeccable historical practice is provided by the way he deals with the question of what can be concluded from Cavendish's laboratory notebook. Watt's supporters, such as Brougham, made a good deal of the fact that Cavendish's notebooks do not contain any claim to draw a conclusion about the composition of water from the experiments they record. Wilson shows through an elaborate analysis of Cavendish's method of working that he *never* entered conclusions in his notebook of such a significant kind. The absence of conclusions about the composition of water is thus normal practice and does not indicate that Cavendish hadn't drawn any conclusions. (See George Wilson, *The Life of the Hon^{ble} Henry Cavendish*, 1851, pp. 366–72.)

his inferences from Cavendish's experiments as imperfectly transmitted via Priestley's attempt to replicate them (and thus give Watt's inferences some basis in sound experimental practice at the expense of attributing to him knowledge of Cavendish's experiments that he didn't have at the time), or we cut Watt's inferences adrift from a sound experimental basis altogether and they become groundless, if inspired, speculations. Either way, the idea that Watt inferred a theory of the composition of water by a sound method is made to look suspect.

Wilson went further than his fellow Cavendish supporters in acknowledging an important problem with this line of argument. Whatever nice distinctions between the conceptual and experimental bases of Watt's views and those of Cavendish can be made in retrospect, these were not known, and in some cases could not have been known, by their contemporaries. So, Wilson seems to concede 'the issue of priority is to be decided by date alone'. In the end, however, Wilson cannot divorce the 'what' from the 'when' because his case for Cavendish having drawn conclusions on the basis of his experiments prior to Watt had to be built on *indirect* evidence. Wilson acknowledged that we do not have any explicit statement of a theory of the composition of water from Cavendish before his published paper. So Wilson's case was based on a hermeneutic analysis of Cavendish's programme of experimental work. Wilson sought to show that the direction, course, strategy and tactics of Cavendish's experiments on air from 1781 to late 1783 *only made sense* if he had drawn conclusions about the composition of water at a stage prior to Watt.[53]

While engaging in these complex hermeneutic arguments Wilson often seems to be adopting a radically contextualist account. He wanted to grant great credit to Watt, whose contemporaries might justly have regarded him as a discoverer in this case. Wilson conceded that Watt assisted in 'inducing belief in the compound nature of water', that his work did service 'at a certain epoch in the progress of discovery, and has a place in the history of science, whether it pleases us that it should have such a place or not'.[54] In the end, however, Wilson believed that the issue of discovery could and should be decided on his own knowledge of the true state of affairs. And that true state of affairs centred most clearly on the sustained experimental programme of Cavendish, and his careful, judicious and accurate method of working:

> Had [Cavendish] never experimented, or had he never reported his results to Priestley, there is no reason to suppose that Watt would have conjectured, even remotely, that water is a compound of oxygen and inflammable air. *He was not on the track of such a discovery*. His speculations on the convertibility of steam into a permanent gas by the change of all its latent into sensible heat, did not point in that, but in exactly the opposite direction. He was following Priestley in all his devious wanderings, and going astray along with him … when Priestley's repetition of Cavendish's experiments (in which all

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[53] See ibid., pp. 317–79. Other evidence is appealed to. If there were no conclusions drawn, then what could Lavoisier have pinched from Cavendish, via Blagden? The latter claims to have made known to Lavoisier in the spring of 1783 not only Cavendish's experiments but also his conclusions. Of course the assumption of bad faith on Blagden's and/or Cavendish's part can undermine this. But Wilson's whole analysis insists on assuming the good faith of all involved. Wilson's life philosophy involved always assuming the best about people.

[54] Wilson, *Life of Cavendish*, p. 437.

> that was true and significant was Cavendish's) arrested him in his mistaken course, and enabled him to approximate to the true theory of the composition of water.[55]

In depicting Watt as 'not on the track' of the discovery of the composition of water, Wilson effectively deconstructed the 'identity claim' made by the supporters of Watt. Their easy assumption that Watt's theory was essentially the same as Cavendish's conclusions was undermined by Wilson's careful depiction of Watt's intellectual world, of the pedigree and context of his ideas. When it came to Cavendish, however, Wilson was not quite so thorough. He did acknowledge that Cavendish remained in the thrall of phlogiston theory. Wilson even conceded that Cavendish saw half of the water in the vessel at the conclusion of sparking experiments as condensed rather than produced. Crucially, however, Wilson contended that Cavendish did regard the other half as produced and therefore genuinely compounded from the original gases.[56] On this basis Wilson argued that Cavendish could claim the discovery. It is important also, however, to recall Wilson's and Harcourt's overall conception of what the discovery of the composition of water consisted in and what its significance was. That significance was not confined to showing that water is a compound rather than an element. It involved the typification of a whole class of chemical reactions and therefore the establishment of the basis of modern chemical practice. On this conception, the discovery could only be properly found in the wider context of a disciplined and sustained programme of research of the kind, and the range, pursued by Cavendish and not so readily attributable to Watt. In this sense, Wilson's whole account of the controversy was predicated on a concern to assert the standards of modern chemical discipline that Cavendish was taken to prefigure. 'Discovery' was treated within that framework of disciplinary concerns and heritage.

Wilson's biography of Cavendish was published, of course, in the year of the Great Exhibition. In the wake of that Exhibition a series of lectures on its results was given before the Society of Arts at the initiative of Prince Albert. Two of these are of particular interest to us. On 26 November 1851, William Whewell spoke on the 'General Bearing of the Great Exhibition on the Progress of Art and Science'. He concentrated on the relationship of Art and Science and noted that with regard to chemical processes and products, 'science has not only overtaken Art, but is the whole foundation, the entire creator of the art'. He continued: 'The great chemical manufactories which have sprung up at Liverpool, at Newcastle, at Glasgow owe their existence entirely to a profound and scientific knowledge of chemistry. These arts never could have existed if there had not been a science of chemistry; and that, an exact and philosophical science.'[57] Whewell having set out in general terms the value of science to Art, most especially in chemistry, the lecture of 7 January 1852 was given by Lyon Playfair, a major architect of the transformation of science education. Playfair addressed 'The Chemical Principles involved in the Manufactures of the Exhibition'. He warmed to the theme, declaring that 'The discoverer of

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[55] Ibid. (my italics).

[56] Ibid., pp. 387–89; J.R. Partington, *A History of Chemistry*, vol. 3, 1962, p. 335.

[57] William Whewell, 'The general bearing of the Great Exhibition on the progress of art and science', in *Lectures on the Results of the Great Exhibition of 1851, Delivered before the Society of Arts, Manufactures and Commerce, at the Suggestion of H.R.H. Prince Albert*, 1852, p. 28.

abstract laws, however apparently remote from practice, is the real benefactor to his kind; in reality, far more so than he who applies them directly to industry.'[58]

> The cultivators of abstract science, the searchers after truth, for eternal truth's sake, are – to borrow a simile, I believe from Canning – the horses of the chariot of industry; those who usefully apply the truths are the harness by which the motion is communicated to the chariot. But is the chariot drawn by the horses or the harness? Truth to say, in this country of ours, – and mark you well, in no other country in Europe, – we honour the harness, but neglect the horses …[59]

Playfair argued that one could examine the work of abstract philosophers and trace from them numerous applications. He then identified Cavendish as the archetype:

> The very impersonification of abstract science was Cavendish … yet, this man, destitute of passions and of sympathies … has by his mind, which still lives, conferred more real material benefit upon Industry than any of the so-termed 'practical' men who have succeeded him. His discovery of the composition of water has given to Industry a vitality and an intelligence, the effects of which it would be difficult to exaggerate.[60]

Playfair had read, and referred to, Wilson's biography. He and Wilson had met in Thomas Graham's laboratory in the 1830s and remained close acquaintances. It was to be partly thanks to Playfair's influence that Wilson obtained his appointments as Director of the Royal Scottish Museum and the Professorship of Technology at the University of Edinburgh. Playfair's lecture shows how the connection was made between Cavendish the abstract philosopher and the burgeoning industrial strength of Britain in the 1850s.

By the time Wilson published his *Life* of Cavendish, the ideological convenience of Cavendish being reaffirmed as the discoverer of the composition of water was enormous. Chemistry was being described (by Whewell among others) as the archetypal science of science-based industry, as *the* example of the power of abstract science as an industrial force. If someone as remote as Cavendish could be convincingly portrayed as having such industrial impact, then the case for the value of abstract science was made. Social and, where appropriate, governmental support ought to follow for the promotion of scientific research and science education, but, crucially, on the terms desired by the leadership of the scientific community and

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[58] Lyon Playfair, 'The chemical principles involved in the manufactures of the Exhibition', in *Lectures on the Results of the Great Exhibition of 1851, Delivered before the Society of Arts, Manufactures and Commerce, at the Suggestion of H.R.H. Prince Albert*, 1852, p. 190.

[59] Ibid.

[60] Ibid., pp. 192–93. The most extreme claim of this type for Cavendish that I have come across was made by Sir William Harcourt, then Home Secretary, on 14 October 1884, when he presented the prizes to students of the central school of science in Derby: 'In chemistry Derbyshire can boast of one of the earliest and one of the greatest of all the discoverers in chemistry … Henry Cavendish, the discoverer of the composition of water … Little could Henry Cavendish know how much that discovery of his would tend to the greatness and the wealth of the country to which he belonged. What he discovered was the foundation to a great degree of all Watt did, and the discovery of the steam engine and the uses of steam which have revolutionized the world' ('Sir William Harcourt at Derby', *The Times*, 15 October 1884, p. 7).

granting autonomy, as well as control of their diverse community, to those pursuing abstract scientific investigations.

Conclusion

I have argued that Cavendish's claim to discovery of the composition of water, and his scientific character more generally, was important to a number of constituencies in early Victorian Britain. He symbolized a kind of fundamental, quantitative research pursued in a thorough, cautious fashion that was important to the scientific self-image of his supporters. The chemists also appear to have adopted Cavendish as a figurehead, and one of their number, George Wilson, constructed a complex chemical rationale for awarding the discovery of the composition of water, that centrepiece of the New Chemistry, to him. Both the ideologues of the British Association and members of the élite chemical community found in Cavendish a useful symbol in negotiating the boundaries of abstract and practical science. By the 1850s the argument was being made more insistently that abstract science was, in the end, the source of the most significant practical benefits and so worthy of national support. Abstract science also needed to be protected from the stifling effects of a focus on immediate practicality. The choice between Watt and Cavendish portended much. In Lyon Playfair's terms, to choose Watt was to honour the harness; to choose Cavendish was to honour the horses.

Chapter 10

The Controversy Joined, 1840–60

Introduction

In preceding chapters we have learned in detail about the various individuals and groups who participated in the water controversy and about the springs and character of their involvement. However, except incidentally in order to help in their characterization, we have not pursued the progress of the controversy proper as it was fought out in private and in public over almost two decades following the release of Arago's *Eloge* and Harcourt's rejoinder to it at Birmingham.

In 1858, Muirhead rehearsed once again the arguments at the heart of the controversy but also ventured a history of the controversy itself. He asserted that, by 1848, when Francis Jeffrey published his article on the question in the *Edinburgh Review*, the controversy was essentially over. In his view it was over and settled in favour of Watt.[1] Muirhead regarded the publication of the *Correspondence* in 1846 as the decisive move. He believed that it removed all obstacles to a general recognition of Watt's priority. Watt Jr had regarded this as his trump card right from the beginning. Muirhead reinforced the idea of the *Correspondence* as decisive by recounting its role in Brewster's 'conversion':

> As an instance of the change which was wrought by the force of the truth on the convictions of others equally distinguished, we may mention a most eminent philosopher [Brewster], who having, at a former period, on the imperfect information then open to him, been disposed to support the claims of Cavendish, on fully studying the fresh evidence which the correspondence of Mr. Watt first made public, unhesitatingly professed his entire conversion; and … publicly announced, as the conclusion at which he had arrived, that the argument for Mr. Watt's priority 'had now been placed on a sound and impregnable basis'.[2]

Unsurprisingly, this view was not shared by the Cavendish camp. We have seen already that Whewell, far from treating the publication of the *Correspondence* as decisive, considered that it did not contain 'any reason for withdrawing' what he had previously stated in his *Philosophy of the Inductive Sciences*. Cavendish's experiments and conclusions, Whewell maintained, conformed to the model of sound scientific practice whilst Watt's decisively did not. This was tantamount to saying that no evidence of a circumstantial kind could ever resolve the controversy

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1 See J.P. Muirhead, *Life of James Watt*, 1858, pp. 379, 383.

2 Ibid., p. 378. We saw above in Chapter 7 that there are reasons to doubt that Brewster was in fact dramatically converted by the evidence of the *Correspondence*. However, it was clearly to the Watt camp's benefit to go along with Brewster's fiction.

in Watt's favour since it had already been settled in philosophical principle for Cavendish. For Whewell, too, the controversy was over, but the *Correspondence* had nothing to do with that situation. Such a stance did not encourage a sanguine view of the likely productivity of debate. Whewell believed that he had the correct philosophical test for identifying discoveries as such. Nothing else was needed.

It is probably true that, as a live debate, the water controversy effectively ceased in the late 1840s. This would help explain why the most substantial treatment of it, George Wilson's *Life of the Hon^ble^ Henry Cavendish*, created little stir on its publication in 1851. Whilst there could be many other reasons for this – it was a long and complex book, not many copies were printed, it was not widely reviewed – it may well be that the controversy was already running out of steam by the time Wilson's book appeared. The clash of interests that sustained the controversy was perhaps attenuating.

So, let us follow our cast of characters from 1839 in their public exchanges and their private reflections in order to ascertain what they saw as the life of the controversy, how their positions changed (if at all), and at what stage the controversy came to be regarded as resolved or no longer of great moment. The main bursts of activity were, first, in the immediate wake of the publication of the *Eloge* and, second, in 1845–46, when a spate of significant publications appeared, most of them triggered by the writings of Henry Brougham.

In the Wake of the *Eloge*

Harcourt's rejoinder to Arago's *Eloge* of Watt came so quickly that many people would have learned of the one from the other. Muirhead's translation of the *Eloge*, and the translation done for the *Edinburgh New Philosophical Journal*, quickly appeared also. The Watt camp moved on a number of fronts. Muirhead was able to develop a brief response to Harcourt in the notes to his translation of the *Eloge*. This was difficult, however, because apart from the testimony of those who had heard the Address, there were only accounts in *The Athenaeum* and in the press to rely upon in reconstructing exactly what Harcourt had said.

In his note Muirhead chided Harcourt for attacking an absent foreigner (Arago) and for failing to engage with the evidence in Brougham's 'Historical Note' to the *Eloge*. Muirhead argued that Arago, as Secretary of the French Academy of Sciences, was by virtue of his character and his position to be 'exempted from all suspicion of indifference to the intellectual glory of his nation'. How much more credible then was Arago when he dropped any claim for Lavoisier in the water controversy and sought to form 'an impartial estimate of the respective claims of two Englishmen'.[3] Muirhead noted that Harcourt's main argument was founded on Cavendish's character and reputation but maintained that, whatever Cavendish's merits as an experimentalist and a philosopher, his behaviour according to the facts unearthed by Arago and Brougham was the only basis for judgement. To stories of Cavendish's diffidence and modesty Muirhead replied with similar accounts of

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[3] François Arago, *Historical Eloge of James Watt … Translated from the French by James Patrick Muirhead*, 1839, p. 114.

Watt's demeanour. He responded briefly to other claims about Watt's view of the nature of phlogiston but otherwise Muirhead professed himself startled at the lack of any evidence in Harcourt's rash production. By this, as we have argued, Muirhead meant the lack of evidence on dates. Harcourt 'not daring to grapple with the priority of publication … expends himself in tedious sophistical declamation on the merits of the respective explanations of their theories'. Finally, Muirhead challenged Harcourt's taste in making his attack in Birmingham, an action that 'has left no impression so strong, as that of general DISGUST'.[4]

Privately the Watt camp speculated about whether Harcourt's attack was a lone effort, or, more likely, they thought, a product of 'conspiracy' involving other leading British Association and Cambridge figures. They also moved to try to secure a friendly review in the *Edinburgh*. We saw that this was unsuccessful. Frustrated by Macvey Napier, the Watt camp had Brougham's note of invective against Harcourt rejected for publication and the review of Arago's *Eloge* entrusted to the unreliable Brewster. The latter, of course, set about pleasing everybody, and his ingenious compromise position published in January 1840, ended up pleasing no one.

Watt Jr and Muirhead had become aware soon after the publication of Arago's *Eloge* that in London the naturalist Robert Brown was taking an interest in the water question, particularly in the aspersions cast on Cavendish's character by Arago and Brougham.[5] Watt Jr appeared confident in the knowledge that Brown had been 'charged by the Cavendish family to prepare an answer'.[6] Brown would have been widely regarded as well placed to investigate the water question. He had, after all, known Cavendish well during his years as Librarian to Sir Joseph Banks and had inherited the charge of the Banksian collections and papers, which he had supervised from his post at the British Museum. Brown had remained active in science. He had supported the cause of reform with the Cambridge men in the 1830 Royal Society election and had received an honorary DCL (Doctor of Civil Law) at the Oxford meeting of the British Association in 1832. By 1839 Brown was an elder stateman of science better placed than most to investigate happenings in the early days of the Banksian regime.

We do know that in October and November 1839 Brown, with the assistance of J.J. Bennett, did a good deal of work on the history of the discovery of the composition of water, the outlines of which can be followed in Brown's papers at the British Library.[7] Brown investigated printed sources of many sorts from the 1780s. In August and September 1839 he had visited Paris and gained access to the register of the Académie, where the meeting at which Lavoisier and Laplace recounted their experiments was recorded.[8] Given the involvement of Blagden in the affair, many would have assumed that the Banks letters would contain information relevant to his contentious activities and therefore hold a key to the question of

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[4] Ibid., note on pp. 115–16.

[5] The following relies on, while clarifying, the account in David Mabberley, *Jupiter Botanicus. Robert Brown and the British Museum*, 1985, pp. 337–39.

[6] Watt Jr to Henry Brougham, 28 November 1839, Brougham Papers, 20,135.

[7] British Library, Add. MSS 33,227.

[8] Ibid., f. 59.

priority and, in particular, whether Cavendish had known of Watt's ideas and taken them as his own.

In the coffee room of the Athenaeum Club in London sometime in mid-October 1839, Brown met Charles Hampden Turner, an old friend of Watt Jr's. Turner was on a mission to discover what light, if any, the Banks–Blagden letters might throw on the controversy. For his part, Brown was interested to see the Watt correspondence on the water question that, as Turner confirmed, Watt Jr was considering publishing. Correspondence at the time reveals that initially Watt Jr was quite happy for Brown to consult his father's letters on the water question, and tentative arrangements were made for him to do so. However, Brown had fallen foul of Arago during his Paris visit. Arago, for whatever reason, was upset that Brown had accessed the archives of the Académie without his (Arago's) knowledge. So, when Watt Jr decided to consult Arago and Brougham on the wisdom of giving Brown the chance to see Watt's water correspondence, Arago suggested that Brown be denied it. A game of cat and mouse ensued, with Brown trying to gain access to correspondence between J.B. Pentland and Aston Hall explaining the reasons for this ban. Turner read some extracts to Brown but refused him a copy of the letter that would have explained Arago's reasons.[9] Watt Jr accepted Arago's and Brougham's advice not to allow Brown to see the Watt correspondence and, importantly, not to publish it at that point. Their concern was a strategic one with which Watt Jr agreed: 'I certainly have all along been of opinion that there will be advantages attending its coming out after our opponents have set forth their case.'[10]

Brown's intervention and his subsequent fading from the scene are interesting in a number of respects. Brown was clear that he became involved only because of the 'moral' question, that is, the charge of foul play against Cavendish. In a statement written after the publication of the Watt Correspondence, Brown acknowledged that if the question had been entirely chemical then he would not have become involved because of his 'imperfect acquaintance' with that science. However, because, with the publication of Arago's *Eloge*, the question became partly a moral one, affecting Cavendish's reputation, he and Bennett became interested in the question, which did not seem to require chemical knowledge beyond their reach.[11] Thus Brown took a rather different tack to Harcourt and Whewell, who were pursuing a primarily chemical argument. Brown was chasing empirical matters of priority, seeming, in fact, to adopt the same model of discovery as the Watt camp members. He invested enormous labour in determining dates, and the movements of key people. Harcourt subsequently relied on Brown for information on such questions. Brown read the proofs of Harcourt's 'Address' and offered advice on dates and interpolations.[12]

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9 Brown recounts his version of events in 'Memorandum written Decr 4th 1839 of the substance of what has passed between Mr Hampden Turner and myself on the subject of the controversy respecting the discovery of the composition of water', Correspondence of Robert Brown, British Library, Add. MSS. 32,441, ff. 336–37 and also see ff. 338–39.

10 Watt Jr to Muirhead, 16 November 1839, No. 2, Muirhead Papers, MS GEN 1354/430.

11 British Library, Add. MSS 33,227, ff. 76–79.

12 See copy of Harcourt to Brown, 8 September 1839 in BL Add. MSS 33,227, ff. 87–88 and Brown to Harcourt, 14 June 1840 [Draft] in BL Add. MSS 33,227, f. 93. It appears also that Brown engaged in what I have called an 'attributional survey' of publications.

Brown had intended to publish something on the water question himself, and there was certainly an expectation among the Cavendish and Watt camps in late 1839 that he would do so. In the event nothing appeared, though Brown did continue to quietly advise Harcourt.[13] It seems likely that Brown acceded to the approach taken by Harcourt and Whewell. By March 1840, Muirhead was stating confidently that Brown had cold feet, that he was 'not known to be doing anything, and is believed to be doing and intending to do nothing. He is considered a cautious man, who will not unnecessarily argue a bad case, or attempt to extract any arguments in his favour, from facts which are all against him.'[14] An aspect of Brown's work did surface publicly, and posthumously in 1859, of which more below. Otherwise, he played no further identifiable role in the controversy.

Aston Hall, meanwhile, looked for reaction to Harcourt from across the Channel. Muirhead was 'full of interest and hope in the approaching crucifixion of Harcourt' in 'our Chief's speech'.[15] It was Arago's account, after all, which had been attacked by Harcourt. Arago, as usual, was threatening dire revenge. He wrote in November invoking the idea of a clerical conspiracy at about the same time as he advised against showing the correspondence to Robert Brown. Arago's main public response, however, came on the occasion of the presentation of a copy of Muirhead's translation of the *Eloge* to the Académie des Sciences. Whilst noting the larger problems with Harcourt's argument, Arago dealt with what he described as Harcourt's two principal objections. One concerned whether Priestley had weighed the gases prior to explosion and the dew produced by it and found them equal. The other matter was Arago's use of the term 'hydrogène' for the word 'phlogistique'. Harcourt had attacked this as a sleight of hand. In response, Arago presented to the Académie an autograph letter of Priestley to Lavoisier, dated 10 July 1782, in which the English philosopher stated the identity of inflammable air and phlogiston. This, Arago contended, showed that the substitution was entirely justified. After this characteristic piece of theatre from Arago, Dumas spoke. He advised the Académie, it was reported:

> qu'après avoir examiné attentivement l'argumentation de son confrère; qu'après avoir fait aussi à *Aston-Hall*, près de *Birmingham*, chez M. *Watt* fils, une étude scrupuleuse de la correspondence de l'illustre ingénieur, il adopte complétement, et dans toutes ses parties, l'histoire que M. *Arago* a écrite de la découverte de la composition de l'eau. 'Mes opinions sur ce point sont tellement arrêtées, dit M. *Dumas*, que je désire voir ma déclaration consignée dans le *Compte rendu* de cette séance.'[16]

Muirhead communicated the substance of this to Watt Jr, commenting triumphantly:

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[13] See drafts and copies of Brown to Harcourt, 7 February 1846 and Harcourt to Brown, 10 February 1846, BL Add. MSS 33,227, ff. 96 and 98, which deal with advice that Brown gave to Harcourt about his paper dealing with Brougham in the *Philosophical Magazine*.

[14] Muirhead to Watt Jr, 28 March 1840, Muirhead Papers, MS GEN 1354/481.

[15] Muirhead to Watt Jr, 17 January 1840, Muirhead Papers, MS GEN 1354/466. The 'Chief' was, of course, Arago.

[16] 'Correspondance. Histoire de la chimie', *Comptes rendus hebdomadaires des séances de l'Académie des Sciences*, **10**, 1840, 109–11.

> *There* is a declaration which will be viewed, throughout Europe, among all the paltry shuffling of Brewster and suchlike, – 'velut inter ignes Luna minores'. Arago, Brougham, and Dumas, are a worthy triumvirate to maintain the certainty of evidence and the rights of your father, against Harcourt, Brewster, and Brown![17]

There was still, of course, no printed version of Harcourt's Address. As we noted, Harcourt had received some assistance from Robert Brown in working the speech up for publication. He received more assistance, though, from William Whewell, as we discussed in Chapter 7. Whewell helped to provide a philosophical framework for Harcourt's argument while the Cavendish family provided him with access to Cavendish's experimental notebooks. The latter gave Harcourt what he regarded as decisive proof that Cavendish had drawn conclusions from his experiments before Watt had his idea. Writing to J.D. Forbes in January 1840, Harcourt, who had felt somewhat under siege immediately after the 1839 meeting, was newly buoyant and confident. This new material would arm him against 'any of your philosophers, seduced in an unlucky hour by the good fare of the Soho works, or by that sympathy which is the one thing, according to Johnson, stronger than truth in a northern bosom'.[18] Harcourt described his valuable find to Whewell in March 1840:

> I have got from the Duke of Devonshire, thro' Lord Burlington, Cavendish's Mss and luckily found among them his day book of the exp^{ts} in question, regularly dated, & shewing beyond further question that one and all, including those by which he first discovered the composition of Nitric acid as well as of water, were made, and the whole investigation completed, between two and three months before Priestley … gave occasion to Watt to speculate on the subject … So preposterous a claim to another man's discovery never was set up, as that which Arago, unhappily for Watt's character for candor, has revived.[19]

Watt Jr and Muirhead saw the published version of Harcourt's 'Address' in late August 1840. They had spent almost a year demonizing him. Now, here, finally was

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17 Muirhead to Watt Jr, 18 February 1840, Muirhead Papers, MS GEN 1354/473.

18 Harcourt to Forbes, 11 January 1840, Forbes Papers, Incoming Letters, 1840, item 2.

19 Harcourt to Whewell, 16 March [1840], Whewell Papers, Add. Ms.a.205$^{138(1–2)}$. The Cavendish family supported Harcourt's efforts. Harcourt had sent a copy of the *Athenaeum* report of his Birmingham speech to Lord Burlington. Returning thanks for it, Burlington advised Harcourt that he had been making inquiries after any Cavendish papers 'which would assist in vindicating his character'. Burlington considered that his family 'ought to feel deeply indebted to you for having vindicated Cavendish's character on so public an occasion; I take great shame upon myself for having been hitherto so passive on the subject, but I hope you will be so good as to let me know if you think I can be of any further use to you, or if any other mode occurs to you in which I might be able to assist in obtaining information'. (Burlington to Harcourt, 28 September 1839, *Harcourt Papers*, vol. 14, pp. 98–99.) William Cavendish (1808–91), Lord Burlington, was educated at Trinity College, Cambridge graduating as 2nd Wrangler and 1st Smith's Prizeman in 1829. He served as MP for Cambridge University from 1829 to 1831 and became second Earl of Burlington in 1834. He served as President of the British Association in 1837–38. Subsequently he was to become the 7th Duke of Devonshire and Chancellor of the University of Cambridge. It is perhaps not surprising that Burlington served as the main intermediary to the family archives recording the work of his distinguished ancestor.

the fully documented argument, including the extensive, and presumably expensive, lithographs from Cavendish's notebooks. Muirhead was not impressed: 'Harcourt's flourish of trumpets led me to apprehend that he had discovered in the MSS so carefully lithographed, something decisive as to the conclusions. But on reading the facsimile carefully over, I cannot find a word of those conclusions in the whole of them.'[20] Muirhead found in the lithographs only proof that Cavendish 'never suspected the real nature of the process' by which water was formed until he heard Watt's deductions. The notebook showed, he claimed, that Cavendish believed in a mechanical deposit of water that he described as 'condensed'. The detail and scrupulousness of the journal meant that if conclusions had been drawn by Cavendish they would be there: 'it is impossible to suppose that notice of the celebrated conclusions would be altogether omitted, if they had existed in Cavendish's own mind at the time during which he conducted the exp^ts^'.[21]

Even as the Watt camp digested Harcourt's efforts, the next meeting of the British Association was imminent. The worthies of Glasgow had been pulling out all the stops. The enthusiasm of the locals threatened to bring ongoing private rumblings over water to the surface again. In December 1839, the Glasgow local committee had added Brougham to their number, in an honorary capacity, and they thought it a good idea to ask him to chair Section A. The locals were apparently unaware that there might be reasons for the volatile Brougham to continue the controversy at the Glasgow meeting. Murchison stepped in and resolved the matter. J.D. Forbes became President of the Section.[22]

Murchison and Edward Sabine, the General Secretaries, gave the Address at Glasgow in lieu of a Presidential Address. They invoked the genius of Watt but left ambiguous the extent to which he was to be seen as a 'man of science':

> raised through the industry and genius of her sons, to a pinnacle of commercial grandeur, well can this city estimate her obligations to science! … she feels how much her progress depends upon an acquaintance with the true structure of the rich deposits which form her subsoil; and great as they are, she clearly sees that her manufactures may at a moment take a new flight by mechanical discoveries. For she it is, you all know, who nurtured the man whose genius has changed the tide of human interests, by calling into active energy a power which (as wielded by him), in abridging time and space, has doubled the value of human life, and has established for his memory a lasting claim on the gratitude of the civilized world. The names of Watt and Glasgow are united in imperishable records![23]

At the dinner given for the Association by the Magistrates and Town Council of Glasgow, the list of toasts included an early one to 'The Memory of James Watt, and the other eminent men of Great Britain who have contributed to the Advancement

[20] Muirhead to Watt Jr, 31 August 1840, Muirhead Papers, MS GEN 1354/502.

[21] Ibid.

[22] Morrell and Thackray, *Gentlemen of Science: Early Years*, pp. 215–16. See also Murchison to Harcourt, 22 March 1840 in Morrell and Thackray (eds), *Gentlemen of Science. Early Correspondence*, pp. 330–31.

[23] 'Address by Roderick Impey Murchison and Major Edward Sabine', *Report of the Tenth Meeting of the BAAS Held at Glasgow in August 1840*, 1841, p. xxxv.

of Science'.[24] The wording of this so as to include Watt among men of science must have caused a little nervousness in some quarters about what else might be to come, though it was safely proposed by General Tscheffkine.

After the Glasgow meeting in 1840 the water controversy seems to have been successfully buried, so far as the meetings of the Association were concerned, for all of fifty years, until Edward Thorpe raised it again at the Leeds meeting in 1890.[25] By then the chief issue was to defend Cavendish's secure claim against a revival of that of Lavoisier.

The stir created by the publication of the *Eloge*, translations of it, Harcourt's 'Address', Brewster's review in the *Edinburgh Review*, and Arago's and Dumas's brief response before the Académie, had died down by late 1840. The next few years were to be a period of relative quiet. This does not mean, of course, that nothing was going on behind the scenes. Watt Jr spent a good part of the period 1840–42 on estate and business affairs. His Welsh estates were expanded and improvements such as planting, fencing and building preoccupied him. This was also the time when the partnership between Watt Jr and M.R. Boulton was dissolved and new partners were taken on. The transition went well and eventually gave Watt Jr more leisure to spend at Doldowlod, but for the moment it was time consuming. The water question took a back seat for a while but was never far from view. Watt Jr appears to have assumed, now that the publication of Watt's water correspondence had been postponed for tactical reasons, that the next step lay with Arago and Brougham. They, after all, were the chief targets of Harcourt's broadside. Brougham had been stymied in access to the *Edinburgh Review* and an effort was made to encourage him to write an article on Watt and water for the *Quarterly Review*. Brougham did write more for the *Quarterly* in the 1840s as his relations with Napier and the *Edinburgh* continued to deteriorate, but the Watt article never materialized. Brougham was unwell through much of the early 1840s, plunged into depression by the death of his daughter.

The lack of a substantial response to Harcourt from Arago and Brougham worried Watt Jr and Muirhead. By 1843 they had decided to make a concerted effort to induce a response from their distinguished friends. In March 1843, Muirhead visited Brougham in London at his Grafton Street house. He was carrying a selection of papers relating to the water question, which he presented to his host. Brougham began to fulminate against Harcourt, Brewster and Davy. He accused Harcourt of 'insolent presumption and folly' in opposing Arago on a scientific question and himself (Brougham) in a matter of evidence. It had been agreed that Brougham, Muirhead and Arago would meet in Paris with a view to the two older men working together to produce, in short order, the definitive response to Harcourt. Muirhead would assist with his knowledge of the case and would provide documents. For his part, Watt Jr would stay at Aston Hall, delaying his journey to Doldowlod, so that he would be able to supply any further documents that might be needed at short notice.

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[24] The list of toasts in reproduced as Figure 23 in Morrell and Thackray, *Gentlemen of Science: Early Years*.

[25] See T.E. Thorpe, 'Presidential Address, Section B', *Report of the Sixtieth Meeting of the BAAS held at Leeds in September 1890*, 1891, pp. 761–71.

Muirhead crossed the Channel with a portmanteau full of books and documents not long after his meeting with Brougham. The latter had asked Muirhead to ensure that Arago was 'primed' for Brougham's arrival a week or so later. Arago seemed enthusiastic, saying that they would 'arranger M. le Chanoine, et nous l'arrangerons bien!' Brougham duly arrived, meetings were held and work was done, but Brougham left before anything was completed. Arago promised to finish the task and send the resulting article for Brougham, Watt Jr and Muirhead to examine. Prior experience with the *Eloge* would not have induced high hopes at Aston Hall and, sure enough, months passed with nothing from Arago. In February 1844 Muirhead was in Paris again, where he saw Arago, who was ill in bed from overwork. That illness, together with Arago's known aversion to writing, Watt Jr believed, would explain the lack of progress.[26]

Brougham decided to go it alone. He wrote to Watt from Chateau Eleanor (named after his daughter) in Cannes on 22 November 1844:

> I have resolved to add a 4th vol. to my Statesmen of Geo III, and to make it consist wholly of men of science and letters. The two first of those of science are my illustrious master Black and my no less illustrious friend your father. I have nearly finished Black and I shall immediately begin the other. If I do not correct all opposition on the controversial points I shall be much disappointed.[27]

Thus was Brougham launched on the work that became the focus of a second period of intense controversy in 1845–46, and that ultimately precipitated the publication of the Watt correspondence on the composition of water.

Sweeping up after Brougham

Early in 1845 Muirhead and Watt Jr were waiting with growing anxiety for communication from Brougham about the life of Watt that he was to publish in his *Lives of Men of Letters and Scienc*e. On 18 March Muirhead reported receiving proof sheets from Brougham. Less that a fortnight later, having conferred with Watt Jr over the proofs, Muirhead arrived in London, heading straight for Brougham's house in Grafton Street. He bumped into Brougham on the doorstep as his Lordship returned to dress for a dinner. The two fell into a heated argument, leaving Brougham late for his evening engagement.

The argument concerned, first of all, Brougham's failure to abide by an agreement that his life of Watt would not be printed without Watt Jr's corrections. Brougham had begun printing before proofs had been sent to Aston Hall. There was also dispute over a number of substantive issues. The most important was the character of Cavendish. Muirhead reported to Watt Jr that Brougham

> Spoke of the perfect honor &c &c of Cavendish: stated that he had already found he was wrong in asserting that Arago had not assailed it: that he has done so in the most

[26] Watt Jr to Brougham, 18 February 1844, Brougham Papers, 29,204.

[27] Brougham to Watt Jr, 22 November 1844, copied in Watt Jr to Muirhead, 3 December 1844, Muirhead Papers, MS GEN 1354/764.

> abominable manner: – that he has no ground for it, does it only because Cavendish was a Lord, is detested throughout Europe and the World for his foul accusations of every one who has a drop of good blood in their veins …[28]

Muirhead and Brougham argued about the insertions in Cavendish's 1784 paper and especially whether Cavendish named Watt and so acknowledged Watt's paper read earlier to the Royal Society. When Muirhead reminded Brougham that the correspondence was 'quite explicit on the silence observed as to yr Father's name at the reading of C's paper', Brougham 'professed to believe that there is no correspondence on the subject which has not been printed'. Muirhead's response was to offer to show the correspondence to Brougham once again, with Brougham's own annotations upon it!

Muirhead was 'certain of the animus with which he [Brougham] now considers the conduct of Cavendish, and Arago has proved right in his prediction. But I shall hope for some effect, – "veteris vestigia flammas" – to be produced tomorrow by his reperusal of the correspondence; and vivid representations shall not be wanting on my part.'[29] The following day, Muirhead found Brougham more receptive and took him through the passages in the correspondence 'conveniently marked by himself' dealing with the water question. In all they spent seven hours on the business. Muirhead achieved his objective in that Brougham agreed to have the whole of the life of Watt reprinted and to do this under Muirhead's superintendence.[30] They also discussed the writing of the life of Cavendish. Brougham had noted the contents of a dozen of the letters in the correspondence and said he considered them most important. Clearly entering into the spirit of the Watt camp again, Brougham suggested that the whole correspondence should be published after this selection had been let out 'as a pioneer, and to draw out anything that may come out from the other side'. Muirhead exulted to Watt Jr

> And where do you think the mine is to explode, but in the very heart of the life of Cavendish, of which I sent you a Proof of the commencement last night!!! Is it not delicious? He fell to work upon it just as I left him tonight and I am very much mistaken if it does not blow Captain Harcourt and all his crew out of the Water.[31]

An examination of Brougham's life of Cavendish reveals, however, that the mine did not, in the end, explode there – this, even though a published promise was made, in a footnote in Brougham's life of Watt, that material from Watt's correspondence would be used in the life of Cavendish.[32]

It appears that although Brougham cooperated regarding Muirhead and Watt's suggested insertions[33] in the life of Watt, he did not follow through entirely as

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[28] Muirhead to Watt Jr, 29 March 1845, MS GEN 1354/791.

[29] Ibid.

[30] Muirhead also had discussions with Brougham's publisher, Charles Knight, who expressed himself glad that Muirhead and Watt Jr had intervened. Knight was worried about Brougham's accuracy and said he would not charge for the corrections (Muirhead to Watt Jr, 3 April 1845, No. 2, MS GEN 1354/800).

[31] Muirhead to Watt Jr, 30 March 1845, MS GEN 1354/792.

[32] See Henry Brougham, *Lives of Men of Letters and Science*, pp. 380–81.

[33] A list of these is appended to Muirhead to Watt, 30 March 1845, MS GEN 1354/792. Muirhead's

intended in the life of Cavendish. Some of the more plainspoken letters were condemned by Brougham as too inflammatory. An example was Black's letter to Watt warning him to beware of 'Blockheads' and 'Rogues' misunderstanding or stealing his discoveries.[34] Muirhead thought that he had persuaded Brougham to include such material but he was still showing resistance to 'moves that affect the Cavendish name'. Overall, however, Muirhead and Watt Jr were happy with the result of their 'management' of the backsliding Brougham. Watt Jr placed the following inscription in Muirhead's copy of Brougham's *Lives*:

> To James Patrick Muirhead, Esq. This volume containing a life of James Watt is presented by his Son, in grateful testimony of zealous & able cooperation in investigating facts and documents, and of critical censure and exposure of attempts to misrepresent & mislead. Aston Hall. 19 April 1845. James Watt.[35]

These custodians of the 'true facts' considered that their management of Brougham was a job well done. By November 1845 Brougham was dutifully submitting proof sheets of his life of Lavoisier to Watt Jr and Muirhead well in advance.[36] Other publications, notably reviews of Brougham, were now clamouring for attention from Aston Hall. These were to precipitate, finally, the decision to publish the *Correspondence*.

The articles in question we now know to have been written by George Wilson and George Peacock, the former in the *British Quarterly Review* and the latter in the *Quarterly Review*.[37] However, these were issued anonymously, of course. The Watt camp were outraged at their contents, but left guessing initially as to the authors' identities. Muirhead learned of the *British Quarterly Review* article in mid-November 1845, finding it 'long and very offensive' and looking 'very much as if Harcourt himself had written the sophistical' piece.[38] Muirhead had immediately recognized the style of argument – one using detailed chemical considerations to challenge the identity of Watt's and Cavendish's conclusions and thus skate over what Muirhead regarded as the key question of chronology. In his view the author

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list also includes the footnotes referring the reader to the Cavendish essay. Watt Jr also lamented Brougham's statement that Watt Sr had studied at the College of Glasgow: 'If there is any one part of my fathers history more remarkable than another and upon which he had more occasion to pride himself, it was that of his being, with the exception of the time he passed with Mr Morgan in Cornhill, entirely self taught upon this head my perfect recollection of the uniform tenor of his conversation leaves me not the slightest doubt, and I trust his Lordship will not contradict him' (Watt Jr to Muirhead, 1 April 1845, MS GEN 1354/796).

34 As reported in Muirhead to Watt Jr, 4 April 1845 MS GEN 1354/801. The Black letter to Watt, 13 February 1783, is reprinted in Robinson and McKie, *Partners in Science*, pp. 123–24.

35 The inscription is copied at MS GEN 1354/811.

36 See Muirhead to Watt Jr, 30 April 1845 and 3 November 1845, MS GEN 1354/815 and 857. Muirhead considered that Brougham's life of Lavoisier was excellent, needed little correction and asserted Watt's claims very well. Muirhead to Watt Jr, 2 December 1845, MS GEN 1354/875.

37 [George Wilson], 'Lord Brougham's *Men of Letters and Science*', *British Quarterly Review*, **2**, 1845, 197–263; [George Peacock], 'Arago and Brougham on Black, Cavendish, Priestley and Watt', *Quarterly Review*, **77**, 1845, 105–39.

38 Muirhead to Watt Jr, 14 November 1845, MS GEN 1354/859.

of the review exhibited ‘the most ignorant, wanton, and foolish disregard of dates and everything else, or the most cunning concealment of their bearing upon the facts of the case … ’. Muirhead believed that Harcourt had supplied the reviewer with ‘his own lying account of the matter’. He did find, however, one, and only one, ‘palliating sentence in all his wretched scribbling’. In this the anonymous reviewer qualified ever so slightly his agreement with Harcourt: ‘With Harcourt’s views in the main we entirely agree, only we think that in his anxiety to vindicate Cavendish, he has done Watt less than justice as a chemist.’[39] The predilection of the anonymous reviewer to do justice to Watt brought him and the Watt camp into an unlikely, almost comical, alliance.

Muirhead wondered whether the anonymous reviewer was worth responding to. After all, the *British Quarterly Review* was a new, relatively unknown, publication with a small circulation and so unlikely to do much damage to the cause. Responding to it might simply bring it to undeserved notice. The situation was complicated, however, because David Brewster had already responded to it and done so in the scientific press, namely *The London, Edinburgh and Dublin Philosophical Magazine*.[40] Brewster took exception to a sentence in the *British Quarterly Review* article that criticized his *Edinburgh Review* essay of 1840 for treating the question between Watt and Cavendish as a matter of national honour and awarding his countryman (that is, Watt) the lion’s share of merit. In reply Brewster vehemently denied being under the influence of nationalistic feelings, and disagreed that he had favoured Watt: ‘Although Mr Watt was my countryman, and my personal friend and correspondent … I have at three different times of my life come to the decision that he was not, and that Cavendish was the discoverer of the composition of water.’[41] To Muirhead this was the most annoying outcome of the episode:

> I confess I should like to have a touch at Sir David, on whom, as assuming to be a judge in the matter, and in place of honouring the name of your father, doing what he can (under the mask of friendship) to disgrace it, I look with continually increasing contempt.[42]

Muirhead now suggested to Watt Jr that the crucial step should be taken – the water correspondence should be published. Brougham had been recommending this since his recent sight of the material. Brougham’s *Lives* appearing first had been intended to bring out any hostilities and had clearly done so. Muirhead suggested that they aim for publication the following spring. Watt Jr agreed and authorized Muirhead to proceed.[43]

No sooner had this decision been taken than the *Quarterly Review* article appeared. This was a paean to Cavendish calculated to hit the Watt camp hard. It is worth

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[39] In Muirhead to Watt Jr, 15 November 1845, MS GEN 1354/862. The sentences quoted from the *British Quarterly Review* article at p. 250.

[40] Sir David Brewster, ‘Observations connected with the discovery of the composition of water’, *The London, Edinburgh and Dublin Philosophical Magazine*, **27**, September 1845, 195–97.

[41] Ibid., p.197. There was a response to Brewster: Anon, ‘Sir David Brewster and the British Quarterly Review’, *British Quarterly Review*, **2**, 1845, 575–78.

[42] Muirhead to Watt Jr, 15 November 1845, MS GEN 1354/862.

[43] Muirhead to Watt Jr, 15 November 1845, MS GEN 1354/862 and Watt Jr to Muirhead, 21 November 1845, MS GEN 1354/864.

looking at activity in the Cavendish camp leading up to this publication. Members of that camp had learned of Brougham's work on his *Lives*. The news may have come from the Duke of Devonshire via Lord Burlington, for in early March 1845 Burlington informed Harcourt that Brougham had applied to the duke for letters and anecdotes of Cavendish to use in the *Lives*. Burlington continued:

> The Duke has no private letters whatever, but has consulted me whether he should let Brougham see the scientific papers. I have told him that I do not think they are what Brougham wants, as I imagine he merely means to write sketches, such as his political characters written a few years ago, and I have recommended him not to send them to him. It has occurred to me, however, whether it might be advisable to shew him the particular papers (of which you made use) relative to the composition of water. He will, of course, have occasion to speak of this; and from the recollection of the part he took a few years ago, he may, perhaps, not be inclined to take a very fair view of the case. An inspection of the papers might tend to remove any false impressions from his mind. I am sorry he has taken up the matter at all, but that cannot be avoided.[44]

The Cavendish camp were expecting a sketch from Brougham and yet also hoping against hope that he might change his mind on the water question after examining complex documentary evidence in Cavendish's scientific papers. But even as they considered this, Brougham was already virtually in print. By early May Peacock had read Brougham's book and his statement on the controversy, and questioned Harcourt as to whether 'anything be more unfair (I had nearly added superficial) than his statement of it?'[45] Spurred by this, Peacock had read numerous documents connected with the subject and entirely concurred with Harcourt's view. He believed that Watt's claim could not be established 'without violation of all the principles of scientific ownership, which have been hitherto recognized'. To do as Brougham had done and to put the 'vague views' of Watt on a par with the 'distinct and unequivocal statements' of Cavendish, was totally unacceptable. Peacock thought that the matter could not be allowed to rest and that a good way to 'make the evidence popular' would be through a review in the *Quarterly*. He volunteered to do this if Harcourt would help with some of the chemistry. In a move reminiscent of the Watt camp's resolve to publish the Watt correspondence on the water question, Peacock also said that he would urge Lord Burlington to publish the chemical, physical and mathematical manuscripts of Cavendish. Peacock felt that Harcourt was the man to do this:

> You have done so much to vindicate Cavendish from the charges which have been prepared at Aston House [*sic*] and presented to the world through Lord Brougham and Arago, that I think you are the proper person to go on with the subject and to put a final extinguisher upon a claim which, in the form in which it has appeared, can do nothing but harm the memory of Mr. Watt.[46]

Peacock duly set to work on his piece for the *Quarterly Review*. By September 1845 he had completed it, but the editor, Lockhart, had advised him that it was

[44] Burlington to Harcourt, 1 March 1845, printed in *Harcourt Papers*, vol. 14, pp. 185–86.
[45] Peacock to Harcourt, 3 May 1845, in ibid., pp. 186–88.
[46] Ibid.

unlikely to appear before July the following year. Peacock was glad of the delay since he had written hastily during a period of illness and depression. He welcomed the chance to revise the piece. His examination of documents had further convinced him that Arago had been precipitate and Brougham just plain ignorant: he doubted whether Brougham ‘ever wrote a single page of a scientific statement without a serious error’.[47] His study of contemporary chemical writers had persuaded Peacock that Harcourt’s analysis of the chemical history was accurate and full. However, he had arrived at an interpretation of Watt’s theory somewhat different from Harcourt’s:

> it appears to me (if I rightly understand his paper, which is not easily done) that his elementary heat was considered as a chemical constituent, and that its action with the same elements in different proportions produced bodies with permanently different qualities. It was his first conception that steam if heated sufficiently, or if imbued with such a quantity of elementary heat that its latent might become sensible heat, would be converted into a permanently elastic fluid: we then find that he supposed in abstract the combination of one portion of elementary heat with oxygen and hydrogen would produce water, with another portion it would produce fixed air: if this view be correct, and I think it is, the pretended announcement of the true theory of the composition of water would be a retrograde and not a progressive movement in correct views of chemical theory.[48]

Watt’s ideas were, Peacock felt, in a confused state because the transformation of bodies one into another occurred through the agency of an element (the elementary heat) that could not be ‘chemically appreciated’.[49]

By October 1845 the publication schedule of Peacock’s review had changed again, and he now lamented the lack of time for him to receive Harcourt’s corrections to the proofs. Peacock advised, somewhat nervously, that he had been very severe on Brougham’s ‘insolence’ and ‘ignorance’, in part because of Brougham’s attack upon Harcourt. Peacock swapped notes with Harcourt, who was now working on his ‘Letter to Brougham’, about Brougham’s failings. He felt that many of Brougham’s mistakes derived from his reliance on Robison’s *Lectures*, which was a ‘singularly inaccurate book’.[50] Peacock also agreed with Harcourt that Watt himself abandoned the claim to the discovery soon after the publication of his paper, and that errant filial piety could explain its resuscitation: ‘most of the recent attempts to revive this claim are referrible [*sic*] to the profuse hospitality of Aston Hall: there is

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[47] Peacock to Harcourt, 24 September 1845, ibid., pp. 188–90.

[48] Ibid., pp. 189–90.

[49] I might observe here that, in terms of my own understanding of Watt’s ideas, Peacock seems ‘spot on’ in his contextual interpretation of what Watt’s notions ‘meant’ at the time they were developed. This does not mean, however, that I want to label Peacock ‘right’ in his participation in the controversy. That is not my role as historian. In any case I would be obliged to label Peacock ‘wrong’ in his blithe assumptions about Cavendish’s ideas and his ‘failure’ to provide a similarly contingent understanding of them. In Mulkay and Gilbert’s terms, Peacock used the ‘empiricist repertoire’ when describing Cavendish’s ideas and the ‘contingent repertoire’ when describing Watt’s. He engaged in selective deconstruction. (See G. Nigel Gilbert and Michael Mulkay, *Opening Pandora’s Box. A Sociological Analysis of Scientists’ Discourse*, 1984, p. 40.)

[50] Peacock to Harcourt, 25 October 1845, *Harcourt Papers*, vol. 14, p. 196. This raises suspicions that Peacock may have been the source of the Cambridge talk about Scotchmen as inaccurate that Daniel Macmillan reported to his friend George Wilson. See Chapter 9.

little doubt but that Arago was seduced by it'. This was typical of the Cavendish camp's view of its opposition as misguided by filial piety, influential through wealth, and scientifically uninformed, or out of date.

Before Peacock's review was published, the editor, Lockhart, intervened to remove passages likely to be offensive to Brougham. Peacock seemed somewhat relieved at this, though there remained much for Brougham and the Watt camp to object to.[51] When we read the review we see that Peacock certainly did hit hard. Cavendish, rather than Black, was given credit for the discovery of 'fixed air' and Cavendish's method of working was lauded to the skies. By contrast Watt's water hypothesis was condemned as 'unprofitable and worthless' and both Brougham and Watt Jr were roundly condemned for their interference. However, a key feature of Peacock's article, which it shared with Wilson's in the *British Quarterly*, was that it brought the chemical discussion to a new level. Specifically, Peacock gave a long history of phlogiston theory and of the relationship of heat theory to chemical thinking. He then claimed to show how, even as Cavendish floated above the mêlée because of his impeccable method and adherence to a non-material theory of heat, Watt mistakenly gave heat a central place in the *chemical* processes he was describing. For Peacock this incorporated into Watt's account 'the phlogiston theory in its most vague and inconclusive form'.

Peacock contrasted his own account, and Harcourt's, with Brougham's and Arago's. He pictured the latter two authors as paying little attention to documents and arguments. Brougham, he says, tossed these aside 'with a sneer':

> he examines no documents, he corrects no errors – but thinks it sufficient to give the sanction of his name to a statement drawn up by Mr James Watt, the son of the great engineer, which is not perfectly correct in the general outline of its facts, and is singularly partial and unjust in the conclusions which it deduces from them. Lord Brougham seemed to have forgotten that much might be pardonable in the fondness of a son which would be highly reprehensible in one exercising the function of a judge.[52]

The Watt camp had received advance notice that an article was to appear in the *Quarterly*. At first they were optimistic because they believed that Murray, the publisher, would not allow anything injurious to Watt's fame to appear. In the very last days of 1845, Muirhead finally received the *Quarterly* in Edinburgh, its shipping having been delayed by violent storms along the coast. When he saw it, Muirhead was outraged, complaining to Watt Jr that the author, whoever he was, was certainly an 'Ass' and 'a Liar, and one of the first water'.[53] They speculated about the authorship. It might be Robert Brown working with Harcourt, or possibly Dr John Davy. Brougham, characteristically, claimed to have inside information about the reviewer:

> I have ascertained that superlative Ass Whewell to be the author of the Q.R. A greater or more notorious blunderer exists not. His numberless books on all subjects he understands

51 Peacock to Harcourt, 8 November 1845, *Harcourt Papers*, vol. 14, pp. 197–98.

52 [George Peacock], 'Arago and Brougham on Black, Cavendish, Priestley and Watt', p. 138.

53 Muirhead to Watt Jr, 27 December 1845, Muirhead Papers, MS GEN 1354/882.

> nothing about, are the ridicule of Cambridge. But a first rate mathematician and friend of his own tells me he never heard him give a lecture on his own subject, without fear & trembling for his inaccuracies, which are quite habitual.[54]

Muirhead, though he must have been mightily amused by Brougham's detection of such faults in another, was not surprised at the news. He recalled that in 1841 Wordsworth had spoken to him of Whewell as 'one of the Carrion crows'. Muirhead was sure that the Reverend gentleman 'would have no objections to be also a "Chanoine d'York". I am afraid your respect for the Cloth is not increased by what we thus learn of its Members'.[55] This anti-clerical sentiment pervaded the Watt camp's discussions of their chief opposition whom they identified as an aristocratic, clerical interest. The implication, of course, was that Whewell was currying favour with the Canon of York and ecclesiastical authorities in the hope of preferment.

Muirhead and Watt sought to rouse Arago, who had been quiet for some time. Muirhead wrote to the Frenchman appraising him of the content of the *Quarterly Review* article with its 'accusations of inaccuracy, partiality, and prejudice' against Arago. Whewell was identified as the author and as the same man

> who nine years ago endeavoured to rob Black of the discovery of latent heat, you [Arago] of the discovery of the phenomena of Newton's rings when produced between a glass lens and a metallic reflector, as well as of the merit of the experiments undertaken at the desire of the French Government on steam at elasticities higher than eight atmospheres.

Whewell had also 'uniformly deprived Scotchmen of any credit (so far as depended on him) for any invention or discovery whatever, and in the whole three volumes of his *History of the Inductive Sciences*, devotes but one sentence to the immortal labors of James Watt'.[56] Muirhead encouraged Arago to reply to these calumnies and also hoped that Dumas would assist the cause by giving them his criticisms of the chemical blunders that Whewell and Harcourt had committed.

Then a further production of the Cavendish camp appeared in the form of the letter to Lord Brougham by Harcourt in the *Philosophical Magazine* in 1846. This response had been encouraged by Peacock, even as the latter worked on his *Quarterly Review* article.[57] It repeated much of the material in Harcourt's 'Address' with, as Muirhead perceived it, 'more of the same sort of stuff, if possible still more confused, drivelling and miserable'.[58] Prominence was given to the charcoal

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[54] Brougham to Muirhead, 28 January 1846, copied in Muirhead Papers, MS GEN 1354/898. Brougham's anti-Whewellian invective mounted as he accused Whewell of 'audacious toad eating of the Archbishop' in his praise of the Archbishop's son Harcourt.

[55] Muirhead to Watt Jr, 30 January 1846, MS GEN 1354/898.

[56] Muirhead to Arago, 2 February 1846, Muirhead Papers, MS GEN 1354/901.

[57] George Peacock to Roderick Murchison, 7 May 1845, Murchison Collection, Geological Society of London: 'I have been attempting to stir up Harcourt to attack Lord Brougham's most unfair & shallow publication.'

[58] W.V. Harcourt, 'Letter to Henry Lord Brougham, F.R.S., &c., containing remarks on certain statements in his Lives of Black, Watt and Cavendish', *Philosophical Magazine*, **28**, 1846, 106–31, 478–525 ; Muirhead to Watt Jr, 5 February 1846, Muirhead Papers, MS GEN 1354/903.

argument, according to which Priestley's 'inflammable air' would have contained carbon monoxide, making it impossible for Watt to infer a 'true' theory. One particular piece of 'insolence' from Harcourt incensed Muirhead greatly: Harcourt described Watt as 'having been unable to examine abstract principles!'.[59] This stung precisely because it depicted Watt as an empiric. Harcourt also, rather cleverly, remarked upon Arago's silence on the water question for a number of years, implying that this was a result of Arago's having been persuaded by Harcourt's arguments in the 'Address'. It thus became important for the Watt camp to elicit some further response from Arago, so as to demonstrate his continued adherence to the cause. The situation was made more difficult, however, because of the falling out between Arago and Brougham.[60] In so far as Brougham was a primary target of the articles by Wilson, Peacock and Harcourt, there was little hope that Arago would leap to his defence.

The trio of pro-Cavendish articles published in 1845 and early 1846 did create a certain pressure on the Watt camp to enter into the chemical issues even as they challenged the capacity of Watt's supporters to do so.[61] There were moves, as we have seen, to enrol the further services of Dumas. Brougham, who was as sceptical of getting anything from Dumas as he was of anything from Arago, suggested that they find an Edinburgh chemist. Muirhead advised Brougham to recruit Professor Thomas Graham of University College London, but there were political difficulties in this. Besides, Graham was inclined to disagree with Arago's statement of the water question. Dr Fyfe at Edinburgh was considered but there were doubts as to his reputability and he had, it was discovered, published in support of Cavendish, albeit very briefly.[62]

Significantly, as this pressure towards involvement in chemical arguments was felt, Muirhead protested to Watt Jr:

> Now entre nous, a chemical analysis of either Whewell or Harcourt, is much more his [Brougham's] business than it is ours. For, as you know, we depend on the dates, and care nothing for absurd endeavours to make two theories which are identical (except as to heat) appear different. When your father said half a dozen times over that by phlogiston he in April 1783 meant inflammable air, I don't care though Harcourt talk of charcoal to

59 Muirhead to Watt Jr, 6 February 1846, Muirhead Papers, MS GEN 1354/905.

60 Arago felt that Brougham had been unsupportive politically. Brougham saw it this way: 'Arago's unaccountable spleen towards me is on account of my not taking part in his crazy politics, and complaining that he lets his crazy brother go about the world levying money on Arago's friends.' (See Brougham to Muirhead, 7 February 1846, Muirhead Papers, MS GEN 1354/906, Copy.)

61 Harcourt concluded his long response to Brougham: 'I hope I may have persuaded you that it is at once more safe, and more just, for those who have not had leisure to pursue chemical studies to their foundation, to leave chemistry and chemists to themselves – at least so far as regards the minutiae of the science, and arbitrations of the rights of discovery … ' (Harcourt, 'Letter to Henry, Lord Brougham', p. 524).

62 On Graham see: Muirhead to Brougham, 10 February 1846 (copied in MS GEN 1354/910); Brougham to Muirhead, 12 February 1846 (copied in MS GEN 1354/915) and 16 February 1846 (copied in MS GEN 1354/918). On Fyfe see Watt Jr to Muirhead, 14 February 1846, MS GEN 1354/915. Andrew Fyfe's *Elements of Chemistry* (1827), as Watt Jr noted, gave the major credit for discovery of the composition of water to Cavendish rather than Watt.

> all eternity; (where he may perhaps find himself getting enough of it, for telling lies and disgracing his holy vocation in this world). As a question of evidence my case is already complete, and I am satisfied to treat it so.[63]

Brougham's situation was different, Muirhead recognized, because he had been attacked directly in both the *Quarterly Review* and the *Philosophical Magazine* and accused of 'numberless errors in the physical sciences wholly irrespective of the water question, and which I should be sorry to see mixed up with it'. Through all this Muirhead had been hard at work on his long introduction to the *Correspondence* and he was inclined to treat that introduction as one thing and Brougham's defence of himself against charges of scientific incompetence as another.[64] Muirhead seemed determined to keep the Watt Camp's case on its firmest ground of circumstantial evidence. However, Watt Jr was keen to assist Brougham, and Muirhead dutifully went along with this. He was eventually to find a person who seemed like the ideal chemist, but one who ultimately caused the Watt camp a great deal of trouble. This was none other than Dr George Wilson.

Wilson swung into the Watt Camp's view by courtesy of a friend of Muirhead's, Dr Allen Thomson.[65] Thomson, according to Muirhead, had 'a great liking for Brougham, considers Whewell a presumptuous puppy, and would do anything in his power to aid the cause'. If Thomson sought an assistant, he knew of a young man, Mr Wilson, who had taken Fyfe's place at Edinburgh and was well spoken of. It was envisaged that the joint labours of Thomson and Wilson as chemists-for-hire would be rewarded by a sum of about 20 guineas.[66]

On the evening of 23 February 1846, in Edinburgh, Muirhead, accompanied by Allen Thomson, met George Wilson for the first time. He reported the meeting to Watt Jr the following day. Muirhead was both startled and gratified by the encounter. He was startled because he learned that Wilson was in fact the author of the article in the *British Quarterly Review*, 'in which Cavendish's claim is upheld, his honor defended, and Lord Brougham assailed on a thousand points with the severest criticism!' Despite this, Muirhead had left the meeting highly gratified because he had 'succeeded in enlisting that active and dangerous enemy in our service, an exploit which has given me the greatest satisfaction, and will I believe greatly aid our cause'.[67]

There were many signs of Wilson's great potential usefulness: he was a man of considerable attainment in science; he had a strong understanding of the history of chemistry; he was regarded by all as highly able and trustworthy; he desired to be impartial. Given that Wilson had, as Muirhead recalled, praised Watt as a chemist

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63 Muirhead to Watt Jr, 9 February 1846, MS GEN 1354/908.

64 Ibid.

65 Thomson (1809–84) was successively in the 1840s Professor of Physiology at Edinburgh and of Anatomy at Glasgow. He was the son of Dr John Thomson, whose translation of Fourcroy, with notes, was published in 1798–1800. On both see *DNB*. As we saw in Chapter 4, these notes sought to set Fourcroy right on the water question. Allen Thomson brought his father's work to Muirhead's attention, who duly noticed it in his Introduction to the *Correspondence*.

66 Muirhead to Watt Jr, 19 February 1846, MS GEN 1354/919.

67 Muirhead to Watt Jr, 24 February 1846, MS GEN 1354/922.

in the *British Quarterly* article, Muirhead thought it worth a try to convert Wilson. A long discussion revealed the young doctor to be fully conversant with the details of the water question. Muirhead believed that Wilson was of a very different mind when the meeting ended. He also learned that Wilson was working on lives of English chemists, among whom he ranked Watt high. Wilson was eager for information, had decided to wait for the *Correspondence*, and agreed to submit to Aston Hall the proofs of any life of Watt that he might write. Finally, Muirhead was convinced that Wilson merely wanted to get at 'the Truth'.[68]

Muirhead wrote to Brougham, not naming Wilson or Thomson, advising that two able men were working on the *Quarterly Review* and *Philosophical Magazine* articles. By now Wilson had become 'our principal chemical operator' and was finding numerous errors in the *Quarterly*. The chemist, Muirhead advised, was 'animated by a cordial detestation of Whewell, whom he considers as blundering as he is presumptuous'.[69] However, both the chemist (Wilson) and Forbes doubted Whewell's supposed authorship of the *Quarterly* article.[70] It was now becoming clear that Peacock rather than Whewell was the author.

As Muirhead spent more time with Wilson he was further convinced that he had converted him:

> There is in fact no difference now between us, excepting that he, like some other chemists, appears still to think it much more probable that Cavendish drew his conclusions in 1781 than that he did not. The grounds on which he is inclined to be of this opinion, are entirely chemical; and he does not feel by any means certain that he is right. The priority of publication, and complete originality on the part of your Father he most fully admits.[71]

Even Watt Jr seemed to overcome his reservations, suggesting that Wilson be shown, confidentially, Watt's correspondence on the water question. He also thought that if Wilson's work proved satisfactory then he might be the ideal person to review the *Correspondence*, once published, for the *Edinburgh Review*.[72]

Over the next few months Muirhead and Watt Jr's attention was focused on the introduction that Muirhead had written to the *Correspondence* and also on a note that Brougham had produced in response to Harcourt and Peacock. Muirhead saw Brougham in London about both documents. He travelled to Paris with Brougham, where he saw Arago. Although Arago was as usual overworked, he met with Muirhead to discuss the water question and assured him that he was as strongly in support of the Watt cause as ever.[73] Muirhead had to deal carefully because Arago

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68 Ibid.

69 Muirhead to Brougham, 25 February 1846, copied in Muirhead to Watt Jr, 25 February 1846, MS GEN 1354/923.

70 Forbes to Muirhead, 28 February 1846 and Muirhead to Forbes, 28 February 1846, both copied in MS GEN 1354/927 (Muirhead to Watt Jr, 28 February 1846).

71 Muirhead to Watt Jr, 26 February 1846, Muirhead Papers, MS GEN 1354/925.

72 Watt Jr to Muirhead, 27 February 1846, Muirhead Papers, MS GEN 1354/926.

73 Characteristically, Arago was full of dire threats that when he next wrote on the question he would be much more severe: 'il faut ecraser, abinier, tous les trois [Harcourt, Whewell and Peacock] je les abimerai tout entier; il faut mettre les pieds sur ces messieurs la, et danser sur ces carrons' (Arago, quoted in Muirhead to Watt Jr, 4 April 1846, Muirhead Papers, MS GEN 1354/951).

and Brougham were still feuding. This could have been disastrous, but Muirhead was relieved that the only effect seemed to be a tendency on Arago's part to condemn Brougham's writings as weak and on Brougham's part a resistance to backing Arago on the question of Cavendish's dishonesty over the interpolations and dates. In Paris, Muirhead also saw Dumas, who professed his continuing support of the Watt cause. Dumas saw nothing resembling conclusions in the facsimiles that Harcourt had published of Cavendish's diary, and even if they had been there, Watt was entitled 'as the first publisher of it in writing, before a learned body, to the priority, on all the principles recognised among scientific men'.[74]

John Murray, who had published the translation of the *Eloge*, was chosen as the publisher for the *Correspondence* on the prospect that he would obtain a better circulation than Charles Knight. The fact that Murray was the publisher of the *Quarterly Review* was not held against him, the editor Lockhart being blamed for letting Peacock's article through. Murray reportedly sided himself with the Watt camp, and felt that Peacock's review had done the *Quarterly* harm. He held out the prospect of a 'counter article' even as he denied any influence over the editor or the *Review*.[75] By June 1846 proofs were being corrected. At this stage the second part of Harcourt's letter to Brougham appeared in the *Philosophical Magazine* and was referred to Wilson for comments but was thought to offer little new, except a lame defence of Peacock's review.

As of September 1846, Watt and Muirhead were monitoring the reports of the British Association at Southampton. They were interested in whether Dumas countenanced the meeting and were glad when it became evident that he was not there. They lampooned what they regarded as the 'Tomfoolery' of the meeting, the admission of ladies as philosophers to swell the coffers, and the lack of gentlemanly behaviour. They rejoiced that *The Times*, too, criticized the Association. In response to Muirhead's criticism of the Southampton meeting, Watt Jr objected to 'the assumption of superior knowledge by their sagacious promoters. The bubble cannot hold together much longer.'[76] Watt Jr and Muirhead no doubt anticipated that publication of the *Correspondence* would contribute to the bursting of the bubble not least by bringing the 'facts' before every reasonable man and revealing the sophistry of the self-appointed élite of the Association.

The *Correspondence* was ready by late November, and Muirhead, Watt Jr and Murray professed themselves happy with it. They began to distribute complimentary

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74 Muirhead and Watt Jr also thought that Brougham's backsliding on Cavendish was a problem. See Chapter 8 above. On Dumas's views see Muirhead to Watt Jr, 6 April 1846, Muirhead Papers, MS GEN 1354/954.

75 Watt Jr to Muirhead, 30 March 1846, Muirhead Papers, MS GEN 1354/946 recounts Watt Jr's meeting with Murray: 'he distinctly denies any authority over or interference with <u>any</u> of the Articles, & said that book after book published by his father and himself had been blown all to pieces, to their great pecuniary loss, but without their venturing to interfere. I however rowed him well for it, and told him it w[d] never have happened in his father's time, which he seemed to think too sharp a cut. We parted good friends, he assuring me that he would do his best to gain the book circulation, and that if the Editor should be satisfied by our publication that the Reviewer [Peacock] had been wrong in either his facts or conclusions, he would readily admit a counter article.'

76 Watt Jr to Muirhead, 26 September 1846, MS GEN 1354/1032, in reply to Muirhead to Watt Jr, 24 September 1846, MS GEN 1354/1029.

copies. The scale of this is revealing. A publisher's account from September 1847 indicates that 170 copies had been sold to that point but more than 200 copies had been given away by Watt Jr and the editor.[77] The first responses came from close allies. Brougham thought the volume 'capital' and was 'amazingly pleased with the figure we cut'.[78] Wilson's reaction was positive. Muirhead described him as 'an enthusiastic convert' and as expressing deep conviction of Watt's originality, priority of publication, and sagacity. Wilson's view of Cavendish's fame had reportedly changed by examining the letters.

> Deprived of the discovery of the comp^{n} of water, he [Wilson] justly says that Cavendish will no longer be looked on as marvellous in the extent either of his observations or deductions; and although I think he has a lurking belief that C. may have drawn the conclusion in his own mind in 1781 or 1782, he is candid enough to admit that we have no evidence … [79]

Muirhead considered this as much as one could expect from any chemist.

J.D. Forbes delayed any reply regarding the *Correspondence*, but Wilson reported meeting him accidentally. Forbes said he was still reading but did not like many of the things that were said about his friend Harcourt. Muirhead was apprehensive since Forbes, as Secretary of the Royal Society of Edinburgh, might use the meeting of that body to make some remarks on the copy of the *Correspondence* presented to the Society. Muirhead decided to attend the meeting prepared to give battle. He would be ready to use an *ad hominem* argument by reminding Forbes 'that in his own discoveries, he has only gleaned where others, greater than he, had reaped. His proceedings as to both Melloni and Agassiz are so well known here, that I shall easily silence him.'[80] Muirhead hoped and believed that the 'popular view' would be that expressed by 'honest John Smith' of Glasgow who thanked Muirhead for the 'lucid statement' in his introduction to the *Correspondence* which, in his view, made it 'impossible to doubt, to whom the world was indebted for the discovery; any more, than the usefulness of bringing these records to light, which establish the righteous claim of the real philosopher'. Relaying Smith's words to Watt Jr, Muirhead rejoiced: 'What would Forbes say if he heard this last, unkindest cut, at his idol Cavendish? On the whole, I see no reason to doubt that the work will be attended with its due effect.'[81]

All awaited anxiously for a response from Jeffrey, who was mysteriously silent. When he finally replied, on 7 December, the delay was explained: the envelope had

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77 Muirhead to Watt Jr, 27 September 1846, Muirhead Papers, MS GEN 1354/1150.

78 Brougham to Muirhead, 3 December 1846, copied in Muirhead to Watt Jr, 5 December 1846, MS GEN 1354/1056.

79 Muirhead to Watt Jr, 7 December 1846, MS GEN 1354/1058.

80 Ibid. The reference to Macedonio Melloni (1798–1854) concerns the closeness of his and Forbes's experimental work on radiant heat. That to Louis Agassiz (1807–73) concerns his controversy with Forbes about their theories of glaciation. See on the latter Frank F. Cunningham, *James David Forbes. Pioneer Scottish Glaciologist*, 1990, pp. 93–110.

81 Muirhead to Watt Jr, 10 December 1846, MS GEN 1354/1061. In reply Watt Jr expressed little surprise at Forbes's behaviour: 'I should as little expect a candid admission of his errors from Harcourt as from his servile admirer. Dr Smith's opinion is worth a hundred such and I trust with you may be the general impression.' I have been unable to identify Dr John Smith definitely.

looked so much like that used to distribute the Bannatyne Antiquarian publications that he had put it on one side unopened! Jeffrey had only glanced at the work but had already been in touch with Napier to remind him of his promise to find a competent reviewer. He asked Muirhead for suggestions. Muirhead, who with Watt Jr had long had Wilson in mind, immediately saw Wilson and Jeffrey. Jeffrey was proposing to ask Sir John Herschel to recommend someone. There was doubt whether Wilson, for all his qualifications (which Muirhead advanced as substantial) was a skilful enough writer for Jeffrey's taste. Eventually, Wilson *was* chosen, but that was only to be the beginning of the story.

Wilson's situation at this point in late 1846 was an intriguing one. His much-vaunted impartiality wove a complex web. Publicly, in so far as his identity as the reviewer in the *British Quarterly Review* (and indeed the article itself) was known, Wilson was identified with the Cavendish camp. Privately he had worked as a hired chemical 'gun' for Muirhead and Watt Jr, and therefore indirectly for Brougham, whom he had criticized harshly in his review. Wilson's work for the Watt camp was to develop a critique of the chemical competence of the articles by Harcourt and Peacock, and thus to strike at the heart of the Cavendish camp's case. Now, even as he negotiated with Muirhead about undertaking the review of the *Correspondence* in the *Edinburgh Review* as the 'nominee' of Watt's supporters, Wilson was approached by the Cavendish Society to write the life of Cavendish for them!

As Wilson told the story to Muirhead, he had been approached to be the Edinburgh agent for a chemical book club being founded in London. He had liked their objectives and suggested that the club be called the 'Boyle Society'. He was surprised to learn, on receiving a prospectus, that the decision had been made to call it the 'Cavendish Society'.

> In September I received a letter from one of the Secretaries, informing me that the Society had some thoughts of asking me to write Cavendish's Life. I wrote in return to say, that unless I was left quite uncontrolled in writing the book, I should not care to undertake it. I have heard no more about it.
> It is manifest, however, that the London Chemists intend to take C's side, otherwise they would not have named their Society after that Observer.[82]

Whilst relations between the Watt camp and Wilson were good at this point, the year 1847 was to see a major saga enacted between Wilson, Muirhead and Francis Jeffrey over the proposed *Edinburgh Review* article on Watt's *Correspondence*. In early December 1846 Wilson was reading the *Correspondence*, by which he professed to profit greatly. On 18 December, however, he wrote to Muirhead having had second thoughts about the wisdom of undertaking the review. Wilson claimed to be in no doubt about the claims set up for Watt, and he believed that his treatment would satisfy Watt Jr and Muirhead in that regard. However, his view of Cavendish's rights in the matter would not. Wilson also could not praise Arago and Brougham

[82] George Wilson to Muirhead, 28 November 1846, copied in Muirhead to Watt Jr, 30 November 1846, Muirhead Papers, MS GEN 1354/1055. This remark supports Brock's surmise about the significance of the name 'Cavendish Society'. See W.H. Brock, 'The Society for the Perpetuation of Gmelin: The Cavendish Society, 1846–1872', *Annals of Science*, **35**, 1978, 599–617.

or denounce Harcourt in the way that Muirhead and Watt Jr would wish. He felt that Harcourt had made important contributions to the debate and that Brougham was blameworthy as a chemist. Wilson was convinced by the Neptune business that Arago was 'not a safe or impartial judge, in a question of scientific priority'. Muirhead replied that so long as they agreed on Watt's claims, that was the main concern. Otherwise he was happy for Wilson to exercise his own judgement and hoped that, if Jeffrey made the offer of the review, Wilson would accept it.[83]

Wilson and Muirhead were on the best of terms as the new year began. Wilson wrote Muirhead a letter of introduction to his old friend George Day, the Secretary of the Cavendish Society, asking him to tell Muirhead all he could about the intended life of Cavendish.

> Mr M. is of course a supporter of Watt, & takes what some of you will consider a strong view of matters. He is however a liberal Critic, and probably the only person in Europe who understands the documentary details of the Watt and Cavendish dispute. Although Mr. Muirhead and I hold different opinions … we both think ourselves entitled to be considered impartial judges & discuss amicably together the points on which we cannot agree.[84]

Wilson even asked that Day keep Muirhead's inquiries a secret from the Cavendish Society people. He sent him as a private friend to learn what he could about the Cavendish biography and 'the feeling in London about the Watt affair'. Wilson assisted in other respects too, advising Muirhead and Watt Jr about how to send copies of the *Correspondence* to key chemist–historians, Frederick Hoefer and Hermann Kopp.[85]

In their next communication, however, Wilson again told Muirhead that he could not write the *Edinburgh Review* article. The reason now was David Brewster's article in the *North British Review*. He shared the concern behind Brewster's treatment of Watt, but felt that justice had not been done to Cavendish. Enumerating the reasons why he should be relieved from the task of the review, Wilson argued first that what he had to say of Watt's merits in the affair would fall short of what Brewster had said. Surely Muirhead would not want 'an uncertain sound' from the *Edinburgh* when the *North British* had sounded 'so distinct & exhalting a note'. His second concern was the polarization that Brewster would rekindle:

> The N.B. article will be highly gratifying to Arago, Dumas, & Lord Brougham: and grievously vexatious to Harcourt and Whewell. It were best to 'let well alone' as I cannot afford like Sir David to make these last transgressors my mortal foes, and I am unable unfortunately to satisfy the first illustrious three.[86]

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83 Wilson to Muirhead, 18 December 1846, Muirhead Papers, MS GEN 1354/214 and Muirhead to Wilson, 19 December 1846, Muirhead Papers MS GEN 1354/215.

84 Wilson to George E. Day, 12 January 1847 (Copy), Muirhead Papers, MS GEN 1354/216(b).

85 Wilson to Muirhead, n.d. [12 January 1847] and n.d. [January 1847], Muirhead Papers, MS GEN 1354/216(a) and 217. Kopp's history was close to completion, and he had already dealt with the water question in volume 3 published in 1845. See Kopp, *Geschichte der Chemie*, 1843–47, vol. 3, pp. 259–72. Wilson thought that Kopp would be interested and might publish a second edition at some stage.

86 Wilson to Muirhead, 1 February 1847, Muirhead Papers, MS GEN 1354/218.

Wilson, as we have seen, was reluctant to alienate the Cambridge men by public disagreement. He was happy to praise Watt, but he would have to defend Cavendish against the charges of theft and falsehood. Muirhead, however, still encouraged Wilson to make himself available to write the review.[87]

In mid-February Macvey Napier died and the editorship of the *Edinburgh Review* was thereby thrown open. Jeffrey now formally took up a caretaker role and it was agreed that Wilson would write the piece. Wilson was immediately inundated with offers of assistance from Muirhead, Watt Jr and Jeffrey! He agreed to have them all correct his proofs. There was a similar offer, via Muirhead, from Brougham also, which Wilson politely declined:

> it were best that his Lordship left me alone, and I do wish you would tell him so. Say I am a dour, perverse, dogged Scotchman, not like him a Giant in Genius, and an Encyclopaedia in learning, but a one-sided Chemist who can not be got to see more than that Oxygen & Hydrogen form water.[88]

In early summer Wilson was still working on the review and exchanged friendly, detailed queries with Muirhead about such matters as the precise publication process for the 74th volume of the *Philosophical Transactions.* Much turned on this so far as the interpolations in Cavendish's paper and the misdating of reprints were concerned. They shared other information. Wilson reported an interview with Jeffrey in which he received valuable advice on the handling of the subject. When Wilson told Jeffrey that although he had different views about Cavendish from Muirhead's estimation, he was not a partisan of Cavendish and had no desire to do injustice to Watt, Jeffrey smiled and replied 'I'll not let you do that.'[89] It is indicative of the spirit of goodwill prevailing that Wilson found nothing sinister or disturbing in this remark.

By early August, a copy of Wilson's manuscript review was in Muirhead's hands, having been sent to the new editor of the *Edinburgh*, William Empson, and to Jeffrey somewhat earlier. This left Muirhead very little time, since the aim at this point was to publish in the October number of the *Review*. Wilson anticipated Muirhead's reaction with some concern since he thought it 'cannot be acceptable to you & Mr Watt'.[90] Wilson and Jeffrey were now trading statements about the balancing of merit between Watt and Cavendish. Jeffrey developed two long statements that Wilson found he had to 'totally dissent' from.[91]

Muirhead arrived in Edinburgh on 18 August and spent that evening preparing a 'full Written statement of the anti-charcoal argument'. The following morning he was with Jeffrey and Empson at Craigcrook expounding that argument:

> They were of course at first disinclined to believe me, in opposition to a professed chemist, on such a point; but I completely satisfied them both, and had the pleasure of

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[87] Muirhead to Wilson, 8 February 1847, Muirhead Papers, MS GEN 1354/219.

[88] Wilson to Muirhead, 27 February 1847, Muirhead Papers, MS GEN 1354/222.

[89] Wilson to Muirhead, 24 June 1847, Muirhead Papers, MS GEN 1354/225.

[90] Wilson to Muirhead, 12 August 1847, Muirhead Papers, MS GEN 1354/229.

[91] Jeffrey to Wilson, 10 August 1847, Special Collections, Edinburgh University Library, Dk.6. 23/1/16/17.

> astonishing, upsetting, and flammergasting these two quick and able men, to my heart's content.[92]

Muirhead was able to inform Watt Jr that Jeffrey would now insist that Wilson alter his paper and, if he would not, then Jeffrey would take the article on himself. Muirhead reassured Watt Jr that 'we shall stand no nonsense from any Doctor on the face of the earth'.[93]

At this point, Wilson, who was at his summer cottage in Dirleton, east of Edinburgh near North Berwick, invited Muirhead to visit him. On 20 August, when Muirhead received the invitation, he arrived that same morning by train from Edinburgh at Drem and was then met by Wilson's gig, which took him down to Dirleton. The two men evidently had heated discussions of the charcoal question – what had Watt known about Priestley's mode of preparing inflammable air? Muirhead did not partake of the country dinner and wine that had been offered, leaving at 3pm. It was shortly after this that Muirhead brought the disagreements to a head:

> It is very evident that notwithstanding all our endeavors to prevent them, misapprehensions have prevailed, with all of us on several important and indeed fundamental points: – not only as to your Sentiments on the great point in dispute but also on the extent to which you feel yourself bound to support preconceived opinions, and so far to be unfitted for the office of an absolutely impartial judge. You now write exactly as I have heard you talk before the publication of Mr. Watt's correspondence: – whereas I had understood that the perusal of that volume, and the repeated Study of the Appendix, had very materially altered your view; – and I certainly was not prepared for the mode of argument adopted in one part of your able and interesting paper, in which I must say, however unwillingly that I do not think that full justice has been done to the claims of Mr. Watt.[94]

Muirhead intimated that any remedy for the misunderstandings might well be beyond reach. He was unwilling to negotiate further and suggested that Wilson sort the situation out with Jeffrey. Wilson's terse reply returned a piece of correspondence between Jeffrey and Muirhead, as requested, and asked that Muirhead send his 'argument against the affirmation of Watt's knowledge that Priestley used the Charcoal gas'. This was important because Lord Jeffrey was adopting Muirhead's views 'but does not state articulately on what grounds'. Wilson felt that he was being denied the information necessary to respond to the criticisms that had created the impasse between the parties.[95] The dispute deepened, Wilson demanding justice, Muirhead expressing outrage at the implication that it had been denied, and

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92 Muirhead to Watt Jr, 19 August 1847, Muirhead Papers MS GEN 1354/1115.

93 Muirhead to Watt Jr, 22 August 1847, Muirhead Papers, MS GEN 1354/1118.

94 Muirhead to Wilson, 27 August 1847, Copy, Muirhead Papers, MS GEN 1354/234. Jeffrey, too, had begun to characterize the disagreement as unmendable: 'What startles me most, I must confess, and quite fills me with misgivings as to the issue of this misunderstanding, is what you now tell me as to the extent to which you are already committed, in honor and consistency, as to the decision of this controversy … I certainly was under a very opposite impression when I first ventured to press you to take this task upon you … '. (Jeffrey to Wilson, 23 August 1847, Special Collections, Edinburgh University Library, Dk.6. 23/1/23–26).

95 Wilson to Muirhead, 28 August 1847, Muirhead Papers, MS GEN 13354/235.

introducing a legalistic tone into the correspondence. Muirhead's demand that no copy of the correspondence between Jeffrey and himself be retained by Wilson was met by Wilson's pointed 'request as a favour' that the manuscript copy of his review article that Muirhead held be returned without any copy in whole or in part being made! Accusations of rudeness began to fly.[96] Amidst all this Jeffrey decided that Wilson's paper was to be withdrawn from the *Edinburgh Review*.

The correspondence between Wilson and Muirhead took a conciliatory turn quite quickly, beginning on 10 September when Wilson sent a long letter 'seeking to remedy the present difference between us'.[97] Wilson rehearsed the course of the disagreement and speculated about why Muirhead had taken offence. He traced it to a disagreement about the charcoal question during their discussion at Dirleton when they both became heated. Wilson assumed that it must have been his request for Muirhead's argument in writing that offended. Wilson explained that he requested this only because he felt that Muirhead's views on the charcoal issue were weighing with Lord Jeffrey's response to the article: 'However greatly Lord J. exceeds us in a thousand particulars, it is paying ourselves no very grand compliment to say, that we know the facts of the water-question better than he does. He looks to us for the facts, & changes his views, as he took the former from you or from me.'[98] Wilson was resigned to Jeffrey's decision not to publish his article, but he doggedly sought to continue the discussion of the charcoal argument with Muirhead. The latter declined, rather wearily, while accepting that the dispute and offence between them were now over. Muirhead forwarded a present of a small writing desk from Watt Jr to Wilson's sister (who acted as a faithful copyist for her brother) as thanks for her efforts in transcribing the manuscript: 'I can wish her its employment in further less thorny themes that that which so lately engaged her pen.'[99]

Muirhead and Wilson maintained friendly, though more distant, contact. Jeffrey and Wilson retained a strong mutual regard.[100] With Wilson taken off the *Edinburgh Review* assignment, an agreement was struck that he would be paid for the article and that Jeffrey would be able to make use of it in his essay. Jeffrey set to work and produced a judicious assessment, though clearly pro-Watt, which appeared in the January 1848 number of the *Review*. We have already examined the content of this piece in some detail. Suffice it here to note that Jeffrey's paper did take a long excursion into the charcoal argument, though only to dismiss it. Muirhead spent some time tutoring Jeffrey in chemical issues to this end. Soon after Jeffrey took the article over, Muirhead described to Watt Jr a session with him:

> I was with him most of the day; and among other things, read over to him, with explanations, Dumas' very elegant paper; from which I showed him that it can be proved, that Cavendish used neither perfectly pure nor perfectly dry hydrogen, nor could the water he obtained have been perfectly pure either. So much for the system of

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[96] Wilson to Muirhead, 2 September [1847], Muirhead Papers, MS GEN 1354/239.
[97] Wilson to Muirhead, 10 September 1847, Muirhead Papers, MS GEN 1354/243.
[98] Ibid.
[99] Muirhead to Wilson, 11 September 1847, Muirhead Papers, MS GEN 1354/244.
[100] Jessie Aitken Wilson, *Memoir of George Wilson*, 1860, pp. 320, 338, 358–59.

> attempting to carry back the chemistry of this century into the last! Wilson's absurd objections can thus be further met.[101]

For his part, Wilson focused his energy and ingenuity upon what was to become the life of Cavendish in which these arcane chemical arguments were to assume an important role.

This saga enacted between Wilson, Muirhead and Jeffrey represented a rapprochement between, then a collision of, opposing models of discovery. Muirhead, against his better judgement that the dates were everything, allowed Wilson to draw him into detailed chemical argument. There, inevitably, he grew frustrated with the fact that Wilson, who had earlier appeared convinced by the circumstantial materials published in Watt's *Correspondence* on the water question, seemed now to have reverted to the position that he had held when they had first met, and Wilson was fresh from writing his *British Quarterly Review* article. Their respect for each other as 'liberal critics' could not, in the end, bridge the conceptual gulf that separated them. In many ways, however, these tense exchanges marked the end of 'live' controversy over the water question. It is to the final ripples that we now turn.

The Controversy after 1848: The Final Ripples

In 1848, C.R. Weld's *History of the Royal Society* was published, the second volume of which contained an account of the water controversy.[102] The account is located in the section dealing with the years 1780–85, but, not surprisingly, draws widely upon subsequent evidence and opinion. It appears that overall Weld's account was designed to pour oil on the waters of controversy. This tendency may well have been assisted by the chemists whom Weld thanked for 'revising the following pages relating to this subject', the chemists being Professors Graham and Miller and Mr Brande.[103]

Weld went out of his way to be even-handed. He did not enter into chemically based judgements. Instead he listed attributions of the discovery made by Arago, Dumas, Brande, Hatchett, Berzelius, Rees's *Cyclopaedia*, Whewell's *Philosophy of the Inductive Sciences*, Peacock and Brown. He tried to take the sting out of some of the key accusations. Thus, while he dealt with the interpolations of Charles Blagden in Cavendish's paper, even going to the length of printing facsimiles of the handwriting of Cavendish, the author of the interpolations, and of Blagden for purposes of comparison, he defused the notion of a conspiracy based on the interpolations. He pointed out that, after the reading of Cavendish's paper, in accord with the then practice, an abstract of it was composed to be read at the next meeting. This abstract, Weld argued, would have been a prime opportunity to fiddle the dates of discovery, but it was not taken. In particular there was no effort to

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[101] Muirhead to Watt Jr, 11 September 1847, Muirhead Papers, MS GEN 1354/1138.

[102] Charles Richard Weld, *A History of the Royal Society, with Memoirs of the Presidents compiled from authentic documents*, 2 vols, 1848, vol. 2, pp. 170–85.

[103] Ibid., vol. 2, p. 170, note 16.

assert the 1781 date of Cavendish's experiments.[104] The import of this was that the actions of Blagden and Cavendish may have involved some genuine mistakes but no intentional fraud or conspiracy against Watt.

At another point Weld dealt with the issue of open scientific communication by discussing not Cavendish's failings in that regard but Watt's. Referring to Watt's withdrawal of his paper and the fact that he did nothing until spurred into action by Cavendish, Weld commented that 'Watt's silence was the extreme of caution'. Weld reported conversations with Faraday in which the latter argued that Watt deserved blame for not communicating immediately a theory that he believed to be true.

> If, for argument's sake it be assumed, that the discovery of the composition of water was of a nature to confer a great and immediate benefit upon mankind, it must be allowed that Cavendish, by his open communication of it to the Royal Society for insertion in the *Philosophical Transactions* … was the first to make it generally known; whereas Watt did not come forward until Cavendish had made the discovery public.[105]

Given Arago's concern with *Cavendish*'s lack of communication, this does smack very much of a desire to balance the scales, to show that there were faults on both sides. Weld's balance was impeccable. He concluded his account by first quoting Whewell's categorical denial of the discovery, and even understanding of it, to Watt, and then immediately endorsing the factual accuracy of the key dates in the affair as set out by Muirhead in his introduction to the *Correspondence*. Finally Weld expressed himself gratified to find 'upon the testimony of the present Mr. Watt' that 'when his father became a Fellow of the Royal Society, in 1785, he formed the personal acquaintance of Mr. Cavendish, and lived upon good terms with him. How greatly is it to be desired that all scientific rivalries might terminate in so amiable a manner!' Weld's clear message was that enough was enough, and the water controversy should be laid to rest.

'The present Mr. Watt', who had in fact shown little sign of wanting to terminate the scientific rivalry, was himself laid to rest at about this time. During the production of the *Correspondence* Watt Jr's failing health was apparent. He was effectively blind and had pain in his legs which affected his mobility. By May 1848 he was in terminal decline and died on 2 June. As the events of the 1848 Revolution rocked Paris, events that his old friend Arago was closely involved in, the affairs of the bachelor who so long ago had participated in the original Revolution were wound up. Brougham received a bequest of books from Watt Jr's library and sought a portrait of his friend, but Muirhead was unable to help with anything except a profile made in Paris in the eighteenth century. The one good portrait of him that existed was still hanging, inaccessible, at Heathfield House.[106] It transpired that Watt Jr had not paid the duty on his bequests. This meant that those receiving them, like Brougham, were left with the bill. Muirhead felt that 'the grace of all Mr W's bequests has been disfigured by the posthumous economy'. He had spoken to Watt Jr about this but the dying man

104 Ibid., p. 173.

105 Ibid., pp. 177–78.

106 Muirhead to Brougham, 8 July 1848, Brougham Papers, 23,387. The then inaccessible portrait was almost certainly that reproduced in Figure 5.1.

had refused to do anything, all to save about £500. Muirhead wondered whether this was a 'money-mania', finding the psychology of it baffling.[107]

The most important bequest from Watt Jr's point of view would have been the charge of caring, as he had done over more than forty years, for the reputation of his father. This fell, inevitably, to Muirhead, who was Watt Jr's literary executor. Muirhead did see through the intended publishing project. The estate, however, passed into the Gibson family in circumstances that caused Muirhead some distress. He recalled for Brougham a trip to Wales 'to Enthrone young J.W. Gibson in his large estates there'.

> I found to my sad annoyance, that at the mature age of twenty-four, he was plucked for his degree at Cambridge, and at twenty-five, celebrated his majority under the Will, by a riotous party of young publicans and cock-fighters from the purlieus of Birm^{m} and Handsworth. And upon this the Venerable Shade was looking down from the wall at Heathfield![108]

It is clear that Muirhead's enthusiasm in the cause had faded somewhat in the 1850s even as he dutifully saw through the publication of *The Origin and Progress of the Mechanical Inventions* and of the *Life*. He probably felt that he owed this much to his old friend and benefactor, Watt Jr, but had no obligation beyond that. Muirhead described the *Life of James Watt* to Brougham as a 'rifacimento'. His literary enthusiasm seems to have been spent on the 'Woodcocks' project and a return to the affairs of the Bannatyne Club.[109]

We saw that Muirhead considered the water controversy to be essentially over after Jeffrey's article was published in 1848. He saw Wilson's publication of the *Life* of Cavendish as an anti-climax. Informing Brougham of its appearance, Muirhead noted its numerous serious misstatements, and detected 'the cloven hoof of Harcourt and Co. into whose hands I was aware that W[ilson] had latterly fallen'. The book, at least 500 octavo pages, was evidently the product of much labour but very imperfect. The imperfections could easily be spelled out, 'should it be thought worthwhile to do so'.[110] The clear implication was that noticing the work might not be worth it. The book was long, 'most unfair, but fortunately also most unreadable' and 'appeared to have fallen stillborn from the press'. To notice the work would only be to draw attention to it.[111] Muirhead thought that unless the Cavendish Society had presented it to the French Institute they should not 'run the risk of propagating error by sending anything to Paris'.[112] Muirhead had believed in

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[107] Muirhead to Brougham, 14 August 1848, Brougham Papers, 23,391. Muirhead did use the term 'psychology'.

[108] Muirhead to Brougham, 16 February 1857 and 26 November 1858, Brougham Papers.

[109] Ibid. The Woodcocks project involved a highly contrived, and rather indulgent, literary venture in which a list of eminent contributors wrote poems and epigrams inspired by Sir Francis Chantrey's feat, in November 1829, of killing two woodcocks with one shot and by Chantrey's sculpture commemorative of the occasion. It was published as J.P. Muirhead, *Winged Words on Chantrey's Woodcocks with Etchings*, 1857.

[110] Muirhead to Brougham, 9 May 1851, Brougham Papers, 3929.

[111] Muirhead to Brougham, 14 February 1852, Brougham Papers, 12,826.

[112] Muirhead to Brougham, 18 February 1852, Brougham Papers, 12,827.

1848 that the controversy was effectively over. Wilson's publication did not seem to change his mind.

Wilson's *Life of Cavendish* was not, in fact, extensively reviewed. None of the major reviews dealt with it. The *British Quarterly Review* carried a favourable notice in its 'Criticisms on Books' section by Robert Vaughan, the editor – no doubt just reward for Wilson's contributions to that journal. The notice printed approvingly a long extract from Wilson's introduction and depicted Wilson as a lone rational voice against the irrationally pro-Watt pronouncements of other reviews.[113] The *Athenaeum* reviewed Wilson's book but concentrated on the issue of Cavendish's character and, so far as the water question was concerned, simply repeated the claim that it had made in reviewing the *Correspondence* – that Cavendish and Watt were to be regarded as joint discoverers who arrived at the discovery independently. This did no justice to Wilson's arguments, but was an understandable response to such a severe and complex book in a general journal of literature.[114] A favourable review, and very clear summary, had been arranged by Wilson's friend, Daniel Macmillan, to appear in his weekly magazine, *The Spectator*.[115] A notice in the *British & Foreign Medico-Chirurgical Review* was positive, praised Wilson's impartiality in the water question but contented itself with long quotations concerning Cavendish's character.[116] Wilson's book was thus not entirely unnoticed, and it did stimulate some other popular accounts of Cavendish, but its impact in the serious literary press was limited. In this sense it is understandable that Muirhead might consider it stillborn.[117]

Another sign that the controversy was regarded as effectively over comes in the form of a meeting between Muirhead and Harcourt in 1856. Muirhead described the occasion, rather jocularly, to Brougham:

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[113] 'Review of *The Life of the Honourable Henry Cavendish*', *British Quarterly Review*, 21, 1851, 257–59.

[114] 'The Life of the Honorable Henry Cavendish', *The Athenaeum*, 13 December 1851, 1305–1306.

[115] 'Wilson's Life of Cavendish', *The Spectator*, **24**, 9 August 1851, 760–61. See George Wilson to Daniel Macmillan, 2 March, 1851, Macmillan Papers, British Library, Add. MSS 55089, ff. 135–36.

[116] 'Review of George Wilson, The Life of the Hon. Henry Cavendish', *British & Foreign Medico-Chirurgical Review*, **9**, April 1852, 533–36.

[117] Among popular accounts, Cavendish's cold, faithless rationalism was a compelling topic. Following Wilson's line, its scientific results were admired even as its human consequences were lamented. For a fine example see: 'The Hon. Henry Cavendish', *The Leisure Hour. A Family Journal of Instruction and Recreation*, **4**, 1855, 489–94. This article awarded Cavendish the discovery of the composition of water (among many discoveries) but also concluded: 'How mournful to think that a man with so many excellences stood aloof from that generous and ennobling faith which would have quickened his dormant affections, and superadded to his intellectual eminence the attractiveness of Christian love' (p. 494). It would be useful to pursue the response to the *Life* into the growing *scientific* press of this period, but I have been unable to do this. It seems likely that Muirhead's confidence would have been dispelled by what he saw there had he deigned to notice that sort of forum. As will become apparent in Chapter 11, a thorough survey of chemical texts would certainly have given him no grounds for complacency. Jessie Aitken Wilson, *A Memoir of George Wilson*, 1860, pp. 341–42 depicted the book as a success and as being widely appreciated as resolving the controversy in Cavendish's favour.

> You will be amused to hear that I have not only broken bread with the Canon of York, (without, however, enquiring into the composition of the other article of prison fare), but that I count some of his family among my dear friends, and was staying with one of them the other day on a delightful visit! To do him justice, he appears to be very amiable, and, so far as I could observe, to bear no malice in his heart. So may all controversies end![118]

After the publication of the *Life of Watt* in 1858, Muirhead's only remaining substantive involvement in the controversy appears to have been occasioned by the death of the botanist Robert Brown.

It will be recalled that Brown had been a rather shadowy worker in the Cavendish vineyard in the wake of the publication of Arago's *Eloge* and Harcourt's response. Brown had in fact done considerable work on the water controversy. The traces remain in his papers, and constitute a very useful resource.[119] It had been expected that Brown would publish something on the water question in 1839–40, but nothing appeared. Brown had consulted a wide range of printed sources and also manuscripts in the Royal Society and the Académie des Sciences. He corresponded with Harcourt and was regarded as something of a behind-the-scenes expert whom Harcourt relied upon for advice on his 1846 letter to Brougham in the *Philosophical Magazine*.[120] Brown was one of the suspects when members of the Watt camp were trying to guess the identity of the author of the 1846 *Quarterly Review* article.

After Brown's death in June 1858, his collaborator and literary executor, J.J. Bennett, felt obliged to release the outcome of the researches on the water question. Indeed, he stated that the executors had received several requests for the release of this information. George Wilson had written that same month to *The Athenaeum* in response to its obituary of Brown. Wilson recounted Brown's interest in the water question and recalled two meetings with him when Brown had referred to documents in his possession which would put Cavendish's claim beyond dispute. When pressed for further information, Brown had simply smiled. Wilson assumed that the documents in question would be among the papers of Sir Joseph Banks that had passed down to Brown. He urged their publication after inspection by interested parties such as the Duke of Devonshire, the Cavendish Society, or himself.[121] In the event Wilson was quite wrong about the nature of the document. Bennett revealed its character and its believed import in a letter to the President of the Royal Society, Sir Benjamin Collins Brodie, which was read to the Society on 17 February 1859.[122]

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118 Muirhead to Brougham, 23 June 1856, Brougham Papers, 3937.

119 See David Mabberley, *Jupiter Botanicus. Robert Brown of the British Museum*, 1985, pp. 337–39. British Library Add. MSS 33,227, 33, 441.

120 Harcourt to Brown, 8 September 1839, 10 February [1840], 9 June [1840], Robert Brown Correspondence, British Library, Add. MSS 33,227, ff. 87–88, 92, 95; draft letters Brown to Harcourt, 14 June 1840, 7 February 1846, Add. MSS 33,227, f. 93, 98.

121 George Wilson, 'Robert Brown and the water controversy', *The Athenaeum*, 26 June 1858, p. 819. Wilson's letter was dated 23 June. The obituary of Brown had appeared in *The Athenaeum*, 19 June 1858, p. 786.

122 '"Statement of Facts relating to the Discovery of the Composition of Water by the Hon. H. Cavendish". In a Letter from J.J. Bennett, Esq., F.R.S. to Sir B.C. Brodie, Bart., P.R.S., dated February 12, 1859. Received February 14, 1859', *Proceedings of the Royal Society of London*, vol. 9, November 1857–April 1859, pp. 642–44; 'Letter from James P. Muirhead, Esq., to Sir Benjamin C. Brodie, Bart.,

Muirhead also wrote to Brodie, on 28 February, rebutting the argument made by Bennett, and he sought to have the letter read to the Society. There appears to have been some reluctance to air the other side of the argument, because it was only after some negotiation that a revised letter from Muirhead to Brodie (dated 8 March) was read to the Royal Society on 10 March. Bennett's and Muirhead's communications were also the subject of notices in *The Athenaeum*, together with a response by George Wilson.[123]

In this exchange the complexities of the arguments in the previous episodes in the water controversy were gone. Bennett's claim was that Brown had made a simple textual discovery that settled the matter. The man who arguably incited the first phase of the controversy in the 1780s by what he wrote to Watt, J.A. De Luc, became the focus of one of the last arguments in the 'live' controversy during the second phase. Specifically, Brown had found in De Luc's *Idées sur Meteorologie* a statement that appeared to show that Cavendish *had* drawn conclusions from his experiments before Watt. Muirhead's response was to set De Luc's words in the context of other things that he said in the same book in an effort to show that their meaning was not as Bennett and Brown claimed. This is an interesting reversal in the sense that the pro-Cavendish forces were now arguing in simple empirical mode whilst Muirhead's was a hermeneutic approach. Wilson's contribution to the exchange reaffirmed the empirical, common-sense perspective.[124]

Pres. R.S., Dated March 8, 1859, relating to the Discovery of the Composition of Water', ibid., pp. 679–81.

[123] On Muirhead's negotiations with Brodie, see Muirhead to Brougham, 7 March 1859, Brougham Papers, University College London, 5527; *The Athenaeum*, 26 February 1859, p. 287 (Bennett); 12 March 1859, pp. 356–57 (Muirhead); 30 April 1859, pp. 582–83 (Wilson). Wilson had previously communicated his views in Edinburgh: 'On the recent vindication of the priority of Cavendish as the discoverer of the composition of water', *Proceedings of the Royal Society of Edinburgh*, vol. 4, November 1857–April 1862, pp. 205–208. (The paper was read on 18 April 1859.)

[124] The arguments focused, according to Bennett, on the date and nature of Cavendish's communication to Priestley. Bennett quoted from the key document, which was De Luc's *Idées sur la Météorologie* (vol. 2, 1787, pp. 206–207). There De Luc recalled visiting Priestley towards the end of 1782. According to De Luc, Priestley spoke of Cavendish's communication to him about the explosion experiments: 'Il me communiqua alors, que M. Cavendish d'après une remarque de M. Warltire; qui avoit toujours trouvé de l'eau dans les vases où il avait brûlé un mélange *d'air inflammable* et *d'air atmosphérique*; s'etoit appliqué à découvrir la source de cette eau, et qu'il avoit trouvé, "qu'un mélange *d'air inflammable* et *d'air déphlogistiqué* en proportion convenable, étant allumé par l'étincelle électrique, se convertissoit tout entier en *eau"*. Je fus frappé au plus haut degré de cette découverte.' The statement within De Luc's quotation marks is taken to be what Cavendish said to Priestley in late 1782, and the words 'se convertissoit tout entier' to reveal that Cavendish believed that the airs were *converted* into water. Bennett regarded this evidence as conclusive of Cavendish's priority. Its credibility, he felt, was enhanced by the fact that it came from De Luc, Watt's 'amie zélé', and that it remained uncontradicted. Muirhead's response to Bennett quoted from other parts of De Luc's *Idées*, in which De Luc's statement could only make sense if Cavendish had made conclusions from his experiments known much later than late 1782. In addition, Muirhead noted that the passage from De Luc that Bennett relied upon was written some years after the encounter to which it refers, and from memory, and so is of doubtful reliability in directly quoting what Priestley told De Luc about what Cavendish had said to him. So much for clear empirical evidence!

We can only speculate about the reasons for this change of approach. It provided the Cavendish camp with a more accessible argument, a feature also of the argument from character increasingly used in the 1850s. Wilson himself was acutely aware of the dryness of his book on Cavendish and that it had not made as much impact as it might otherwise have done. Even experts in the field would have found some of Wilson's (and Harcourt's) historical arguments difficult of access. Learned chemists were increasingly removed from the era of phlogiston theory and would not necessarily have had it at their fingertips as an object lesson in bad science. This was one sign that the water controversy was becoming a truly historical problem by the 1850s, that is, a problem not so immediately driven by contemporary concerns.

Another indication of the declining saliency of the controversy is that opportunities to argue the respective cases were no longer so used. This was true of reviews of *The Origin and Progress of the Mechanical Inventions*, which was published in 1854, as well as of reviews of the *Life of James Watt*, when it appeared in 1858. Brewster's review of the former work did maintain Watt's claim on the water question but did so simply by referring back to his 1847 article.[125] Reviewers were more concerned with an overall assessment of Watt and his significance. They were happy to see him as an engineer, albeit a philosophical one. The urgency to make him a philosopher and a scientific discoverer was no longer so great. The case of Samuel Smiles's treatment of Watt is instructive in this regard.

Smiles contacted Muirhead in early 1858 via his publisher, John Murray, unaware that Muirhead's *Life of James Watt* was about to be published. He informed Muirhead that he had done considerable work on a life of Watt and requested access to the correspondence. He did not want to quote from it but 'to gather from the inspection of the letters, such traits of character & points of interest in reference to personal matters as often let in a flood of light on individual history & its development'.[126] Muirhead's reply was sharp and unhelpful:

> It is, I feel assured unnecessary for me to remind you that I am not only a kinsman of Mr Watt, but the literary Executor of his son; and I do not hesitate to say, that no complete or worthy Life of James Watt can ever be written by any one who is disbarred, as you would be, from the free use of both his Correspondence (whether published or in MS) and the Specifications of his Patents.
>
> Under these circumstances you will no doubt be ready to admit that the subject of your projected undertaking is already very decidedly and legitimately pre-occupied.[127]

Smiles retired calmly and courteously in the face of this rather curt rejection.[128] However, when Muirhead's *Life* appeared, Smiles took the opportunity to review it anonymously. His criticisms are of considerable interest as a discussion of how the life of a man such as Watt should be approached in the high noon of Victorian society. Smiles, perhaps getting his revenge, was critical of the hyperbole that

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125 David Brewster, 'Review of Muirhead's *The Origin and Progress of the Mechanical Inventions of James Watt*', *North British Review*, **23**, 1855, 193–231.

126 Samuel Smiles to J.P. Muirhead, 4 January 1858, Muirhead Papers, MS GEN 1354/91(a).

127 J.P. Muirhead to Samuel Smiles, 7 January 1858, Muirhead Papers, MS GEN 1354/91(b).

128 Smiles to Muirhead, 9 January 1858, Muirhead Papers, MS GEN 1354/91(c) and John Murray to Muirhead, 5 February [1858], Muirhead Papers, MS GEN 1354/91(d).

Muirhead indulged in. He regarded it as inappropriate in any biography, but especially in Watt's case: 'The life of that sterling and thoroughly unaffected man ought to be a sterling and thoroughly unaffected book.'[129] Apart from attacking Muirhead's style, Smiles criticized the tendency to exaggerate the, admittedly colossal, impact of the Watt engine and to attribute the steam technology of the railways to Watt, when it belonged to George Stephenson. In another review attributed to Smiles, other 'corrections' were made, for example, to over-enthusiasm in discovering childhood genius in Watt and to the depiction of the idea of the separate condenser as a flash of inspiration rather than the culmination of a lot of hard work. Smiles gave the composition of water a sentence: 'it is now placed beyond a doubt that he was the first to promulgate the true theory of [water's] composition, though Cavendish had arrived by independent research at the same result'.[130] Here, as in the *Lives of Boulton and Watt* eventually published in 1865, Smiles gave little prominence to the water question and, when he mentioned it, pushed controversy firmly into the past while retaining a judicious, non-committal claim for Watt.[131] Stories about Watt were taking their place in Smiles's pantheon of heroic, human, and not necessarily scientific, engineers. Smiles was fond of Watt's garret workshop. It was illustrated in his *Lives of Boulton and Watt* and represented as the site and symbol of Watt's creativity. In 1874 Smiles wrote:

> One of the most remarkable things about engineering in England is, that its principal achievements have been accomplished, not by natural philosophers nor by mathematicians, but by men of humble station, for the most part self-educated … The great mechanics … gathered their practical knowledge in the workshop, or acquired it in manual labour.[132]

In the struggle between those propagating the narratives of 'genius' and of 'hard work', so active at this time, Watt the craftsman and engineer gained ascendency over Watt the philosopher.

In 1875 the chemist–historian T.E. Thorpe delivered a lecture on Henry Cavendish in the Manchester Science Lectures at Hulme Town Hall. When Thorpe came to what he called Cavendish's 'greatest discovery' he looked back on the water question.

> Not many years ago there was a great controversy concerning the question – Who was the discoverer of the composition of water? I am not now going to rake up the matter; for it is gradually being forgotten; but I think that every chemist now allows that the claims of Cavendish have been incontestably proved.[133]

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129 Anon, 'James Watt', *Fraser's Magazine*, **59**, 1859, 318. This is, I think, attributable to Smiles.

130 [Samuel Smiles], 'Review of Muirhead, *Life of James Watt*, Muirhead, *The Origin and Progress of the Mechanical Inventions of James Watt* and George Williamson, *Memorials of the Lineage, Early Life, Education, and Development of the Genius of James Watt*', *Quarterly Review*, **104**, 1858, 447.

131 See Samuel Smiles, *Lives of Boulton and Watt*, 1865, pp. 377–81. Smiles states: 'Each was quite competent to have made the discovery; nor is it necessary for the fame of either to strip a leaf of laurel from the brow of the other. Moreover, we are as unwilling to believe that Cavendish would have knowingly appropriated to himself the idea of Watt, as that Watt would have knowingly appropriated the idea of Cavendish' (p. 380).

132 Quoted in Eric Robinson and A.E. Musson, *James Watt and the Steam Revolution*, 1969, p. 1.

133 T.E. Thorpe, 'Henry Cavendish', in T.E. Thorpe, *Essays in Historical Chemistry*, 1902, p. 95.

Thorpe was born in Manchester in 1845, when the water controversy was at its height. He studied under H.E. Roscoe at Owens College and then with Robert Bunsen at Heidelberg. The Chair of Chemistry at the Andersonian College, Glasgow, was his first appointment. In 1874 he moved to a chair at the new Yorkshire College of Science in Leeds. Just thirty years old when we meet him, he was a prime example of the thoroughly trained, actively researching, institution-building chemist of the last quarter of the nineteenth century.[134]

Thorpe was happy to see the water controversy forgotten and was convinced that the case for Cavendish had been victorious. He did note, however, that 'the time was ripe for this discovery'. The work of a number of Cavendish's contemporaries was tending, Thorpe said, in the same direction and, if Cavendish had not capitalized on his opportunities, then someone else would have made the discovery with little delay. Appealing thus to the phenomenon of simultaneous or multiple discovery, Thorpe effectively rendered the discovery of the composition of water as the product of the culture of late eighteenth-century chemistry. In this way he took the sting out of any contest and justified his decision not to rake it up. The chemical consensus on the water question went hand in hand with Thorpe's assessment of Cavendish's overall significance. Considering Cavendish's corpus of scientific work, Thorpe asked rhetorically 'what is the most obvious characteristic of all this labour?'. The answer was its thoroughly quantitative character:

> Weighing, measuring, calculating; such, indeed, was pre-eminently the essential nature of Cavendish's work. If, then, the claim of any one to be styled the founder of chemistry as a science rests upon his recognition of its quantitative relations, may we not also, and with equal truth, say that 'Chemistry is an English Science – its founder was Cavendish, of immortal memory'?[135]

This was, of course, a response to Adolph Wurtz's famous statement of 1869 in which he claimed chemistry as a French science whose founder was Lavoisier. Clearly, the iconic status of Cavendish remained very important to Thorpe and to many of his fellow British chemists in the later decades of the nineteenth century. Cavendish's claim to the discovery of the composition of water was secured, finally, by his identification with the very nature of the science of chemistry. That discovery was a central feature of the science. Chemistry was defined as what Cavendish did. Ineluctably, the discovery was his.

Thorpe defended this position against the claims of Berthelot, published in 1890 in his book *La Révolution Chimique, Lavoisier*.[136] This episode had interesting parallels with earlier events. Here was a Perpetual Secretary of the Académie, one

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[134] Thorpe went on to have two spells as Professor of Chemistry at the Royal College of Science, South Kensington (1885–94 and 1909–12), which sandwiched his tenure of the post of Government Chemist. He became FRS in 1876 and was President of the Society of Chemical Industry in 1895, of the Chemical Society (1899–1901) and Foreign Secretary of the Royal Society (1899–1903). His research concerned vanadium, phosphorus compounds, the relation of molecular weights to specific gravities, and the accurate determination of atomic weights. He was also to become well known for his work in the history of chemistry. (*DNB* (1922–30), pp. 842–43).

[135] Thorpe, 'Henry Cavendish', p. 97.

[136] M. Berthelot, *La Révolution Chimique, Lavoisier*, 1890, ch. 10.

of Arago's successors making strong claims on the water question, but this time in favour of Lavoisier. Thorpe, who was President of the Chemical Science Section of the British Association, responded at the Leeds meeting of that body in September 1890.[137] Berthelot essentially took the position that Lavoisier himself had taken in 1783, implying a smooth progress of Lavoisier's experimental programme gaining little from the news brought to Paris of Cavendish's experiments. Thorpe in response pointed to Lavoisier being on the wrong track, having abandoned phlogiston only to be led astray by *le principe oxygine* in looking for an acid as product of the combustion of inflammable air. Only the detailed news of Cavendish's experiments put Lavoisier back on track. Thorpe reproduced Blagden's letter to Crell's *Chemische Annalen* through which Blagden and Cavendish sought to set things right at the time. Thorpe noted that Lavoisier never responded to Blagden's letter, thereby tacitly accepting Cavendish's priority. So once again the water question was before the Association, some fifty years after Harcourt's 'Address', this time to defend Cavendish against Lavoisier rather than Watt.

On 11 March 1898, Thorpe delivered the Watt Anniversary Lecture to the Greenock Philosophical Society in the Watt Memorial Hall, the very building that Watt Jr and the worthies of Greenock had erected to Watt's memory. Thorpe was clear from the beginning that Watt was to be regarded as a 'scientific man, in the truest and noblest sense of that term'.[138] Before this audience, Thorpe ended up dividing the spoils: 'these eminent men [Watt, Cavendish and Lavoisier] took an independent and, we may say, an equally important share in the establishment of one of the greatest scientific truths that the eighteenth century brought to light'. The lecture was carefully judged in that its conclusion praised Watt's 'intellectual grasp', his 'mental vision' and his 'love of truth' even as the body of the lecture tied Watt's thinking on water firmly, more firmly so than Cavendish's, to old modes of thought. Thus in the conclusion we are told that Watt 'was the first, so far as we can prove from documentary evidence, to state distinctly that water is not an element, but is composed, weight for weight of two other substances, one of which he regarded as phlogiston and the other as dephlogisticated air'.[139] However, Watt reasoned from 'imperfect and altogether erroneous data' whilst Cavendish supplied the 'true experimental basis' of the discovery.

In the body of the lecture Thorpe argued that Watt's early ideas on the interconvertibility of air and water were grounded in ancient notions, a kind of intellectual hangover. Moreover, there were key continuities between Watt's early ideas on interconvertibility and those which he announced in his letters of 1783 and the *Philosophical Transactions* in 1784. The key continuity was that Watt still thought in terms of 'air' as a single sort of matter and that the different airs with which pneumatic chemistry was populated were distinguished by different modes

[137] T.E. Thorpe [Presidential Address, Section B], *Report of the Sixtieth Meeting of the British Association for the Advancement of Science held at Leeds in September 1890*, 1891, pp. 761–71, especially pp. 766–71.

[138] Thorpe, 'James Watt', in *Essays in Historical Chemistry*, p. 101. The lecture was printed, and its venue identified in 'James Watt, and the Discovery of the Composition of Water', *Nature*, **57**, 7 April 1898, 546–51.

[139] Ibid., 121.

of composition with heat. This was essentially the view of Watt that Peacock and Harcourt had expressed. Thorpe thus depicted Watt as doubly in thrall to phlogiston theory and to a material chemical theory of heat. Cavendish himself, Thorpe conceded before his Greenock audience, remained in the grip of phlogiston theory. The same point was not emphasized, however, in the account of Cavendish given in the Manchester Science Lectures. Even as the iconic status of Cavendish for the chemical community was maintained, concession to Watt's claims was still possible before other audiences as the occasion required. As the century closed, the calm modesty and love of truth exhibited by Watt could be maintained, even though, as Thorpe put it, 'the voice of envy and detraction has not been unheard amongst the strife of partisans in the Water Controversy'.[140]

Explicit controversy over the water question had certainly calmed by the 1860s. It had run out of ideological steam. The attributional process, of course, continued and we conclude the substantive examination of the controversy with our second attributional survey. We will see that the victory that Muirhead and Brewster felt had been won for Watt in the realm of the reviews was lost sight of in a wider attributional arena.

140 Ibid., 122.

Chapter 11

Still Waters: Attributional Survey, 1830–1900

Introduction

It was noted in connection with the first attributional survey in Chapter 4 that the line between the controversy proper and the ongoing processes of attribution is difficult to draw. In principle it is impossible to draw, since there is no line. The activities involved in the controversy itself are primarily processes of attribution. The difference, then, is one of perception. We might safely say that during the late 1830s and through the 1840s a literate observer of the British scientific scene would be aware that a controversy on the water question was going on. Books and pamphlets were appearing and articles in the reviews were carrying the arguments on. By the mid-1850s, however, it is not clear that the same literate observer would be aware of a current controversy. Relatively still waters prevailed, but there were undercurrents. As public controversy calmed, a number of beneath-the-surface moves were made in texts and other publications that, while not very visible contributions to an ongoing controversy, were important in the settling of the issue in an attributional sense. That settling process stretched out over several decades. Let us examine first what was happening in the important area of encyclopaedia coverage of these issues.

Encyclopaedic Interventions

The importance of the *Encyclopaedia Britannica* in the early history of the water question will be recalled. Articles on 'Water', 'Steam' and the 'Steam Engine' in the third edition were important in emphasizing Watt's philosophical character and gave him considerable credit for discovery of the theory of the composition of water. Thomas Thomson's articles on 'Chemistry' from the 1801 *Supplement* onwards redressed the balance very much towards Cavendish while still giving some acknowledgement to Watt. The key decision by Macvey Napier to invite Watt Jr to write the article on his father for the *Supplement* to the fourth, fifth and sixth editions of the *Britannica* created, as we have seen, an opportunity for the Watt camp's claims on the water question to reach the *Britannica*'s audience. Other encyclopaedias before 1830 commonly divided the credit for the discovery between Cavendish and Watt, though inclining towards Cavendish. Consistency was not a strong virtue of these publications, and contradictions between different articles in the same encyclopaedia were quite common.

The seventh edition of the *Britannica* was also edited by Napier but published by a new owner, Adam Black, who had purchased the *Britannica* from Constable for £6150 at auction in the late 1820s. Although Black had hoped to publish the new edition by 1830, it was not until 1842 that its twenty-two volumes were completed. These volumes retained many articles from earlier editions. Thomson's article on 'Chemistry' remained in a further revised form. It had less historical content than previously. The brief historical section firmly identified Cavendish as the discoverer of the composition of water.[1] No mention at all was made of Watt. Water was dealt with briefly and abstractly under 'Hydrogen', without history or attributions. The article on Cavendish was substantially unchanged from earlier editions, remaining largely a seriatim account of Cavendish's papers in the *Philosophical Transactions*. The article clearly and unequivocally characterized Cavendish's 1784 paper as containing the discovery of the composition of water. It also congratulated the British chemists for not being seduced by system: 'it must ever be remembered, to the honour of Mr Cavendish, and to the credit of this country, that we had not all been seduced, by the dazzling semblance of universal laws, to admit facts as demonstrated which were only made plausible by a slight and imperfect analogy'.[2] The article on Watt was still that written by Watt Jr, with its claim that Watt was the first to make known the theory that 'water is a compound of dephlogisticated and inflammable airs … deprived of their latent or elementary heat'. The only textual differences are that in the concluding note Watt Jr is identified as the author of the article, and the reader's attention is drawn to Arago's recent 'admirable account' of Watt.[3] Thus, in the edition overall, mixed messages were still conveyed, although the strictly chemical articles had nothing to say about Watt.

Macvey Napier died in 1847 and in 1851 Black decided to publish a new edition, the eighth. Dr Thomas Stewart Traill, Professor of Medical Jurisprudence at Edinburgh University, was chosen as editor. John Leslie's dissertation on the progress of the mathematical and physical sciences was continued by Professor James David Forbes. In that dissertation Forbes was to change substantially the way in which the *Britannica* represented the 'water controversy'.

In some respects inertia still ruled the day in the eighth edition. The article on Cavendish was virtually unchanged. That on Watt was also substantively the same. However, an effort had been made to direct readers to counter-balancing material. A note at the beginning of the article on Watt clearly identified Watt Jr as the author, stated that some changes had been made in the article, and directed the reader to remarks on Watt in Forbes's 'Preliminary Dissertation'.[4] It will be

[1] Thomas Thomson, 'Chemistry', *Encyclopaedia Britannica*, seventh edn, vol. 6, 1842, pp. 341–505. For the attribution to Cavendish see p. 349.

[2] 'Cavendish, Henry', ibid., pp. 262–63.

[3] 'Watt, James', ibid., vol. 21, 1842, pp. 818–19.

[4] 'Watt, James', *Encyclopaedia Britannica*, eighth edn, vol. 21, 1860, p. 773. The note reads: 'It may be interesting to the readers of the *Encyclopaedia Britannica* to know that the following article on JAMES WATT was contributed in 1823 by the son of the great mechanician, the late James Watt of Birmingham. A few necessary emendations have been made on it, chiefly drawn from *The Origin and Progress of the Mechanical Inventions of James Watt*, by his kinsman, James P. Muirhead, M.A., 3 vols., 1854; and from the *Life of James Watt* by the same author, 1858. The character of Watt appended

remembered also that Watt Jr had inserted a footnote in his original article referring to the confusion of dates in the papers in the *Philosophical Transactions*. In the eighth edition this footnote was carefully modified. The reference to Cavendish having circulated misdated copies of his paper was removed, as was Watt Jr's arch commentary. Instead we are told that the reason for the confusion has been cleared up and we are referred to Muirhead's *Life of James Watt* for details.[5] The obedient reader who turned to Forbes's 'Preliminary Dissertation' would find there a substantially changed picture of the water question.

Forbes was, as we have seen, a member of the gentlemanly élite of the British Association who cooperated with Baily on the Cavendish experiment, regarded Cavendish as a scientific hero, and supported Harcourt's defence of Cavendish at the Association's Birmingham meeting in 1839. In the 1840s and 1850s, rather quietly, Forbes published a number of items that began, as he saw it, to set the record straight in various respects.

In writing a life of John Robison for the Royal Society of Edinburgh,[6] Forbes had studied Robison's articles in the *Encyclopaedia Britannica* on 'Steam' and 'Steam Engine'. It will be recalled that those articles had given great credit to Watt, but that the engineer himself had subsequently edited them to assert his lack of indebtedness to Joseph Black in working through to the idea for the separate condenser. In 1855, when he was working on the section dealing with Watt in his 'Preliminary Dissertation', Forbes sought comments from George Wilson. Wilson encouraged Forbes to reassert Watt's debt to Black, thus, in his view, setting right both Watt himself and Muirhead, who had perpetuated Watt's view in his introduction to the *Correspondence*. He wrote to Forbes, 'your note corrects this revival of Watt's own misconception of his debt to Black … '.[7]

On the water question itself, Wilson felt that it was 'much better the Controversy … should be left untouched in Watt's Life'. This was because 'no praise of Watt will satisfy those Eulogists of his who have made his superiority to Cavendish the subject of contention till the necessity of subordinating the latter has become a monomania with them'. Wilson suggested that Forbes might notice with additional fullness Watt's knowledge of the chemistry of his time. He was, after all, a friend of Black and Priestley, helped to introduce chlorine bleaching, and was an experimenter himself. For these reasons Watt 'had claims to be called a Chemist as the recognized

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to the biography is from the brilliant pen of the late Lord Jeffrey, who knew the engineer well, and enjoyed much of his esteem. Further information regarding Watt and his discoveries will be found in the SIXTH PRELIMINARY DISSERTATION, by Principal Forbes, prefixed to this work.'

5 The note reads: 'There is a confusion of dates in the accounts of this affair. Mr Watt's letter to M. de Luc in the *Philosophical Transactions* appears dated 26th November 1784, which is evidently an error of the press. Mr Cavendish, in his letter, read 15th January 1784, speaks of Mr Watt's paper "as lately read before the Society", whereas the paper itself purports to have been read on the 29th April 1784. [This confusion has since been cleared up by the discovery that the 26th November 1784 of the *Philosophical Transactions* should be 26th November 1783. See p. 343 of the *Life of James Watt*, by J.P. Muirhead, London, 1858.]'

6 'Biographical Notice of Sir John Robison', *Proceedings of the Royal Society of Edinburgh*, vol. 2, 1846, pp. 68–78.

7 George Wilson to J.D. Forbes, 17 October [1855], Forbes Papers, Incoming Letters, 1855, no. 128.

coadjutor of the Chemists of his day'.[8] Thus Wilson and Forbes sought to shape the chemical reputation of James Watt, at once granting his chemical credentials in his own time and yet, by depriving him of the discovery of the composition of water, denying him a place in the making of the New Chemistry.

Forbes, it must be realized, greatly admired Watt. He regarded him as a 'profound' engineer. It is worth dwelling on this terminology for a moment. It was important to Forbes that specialization be recognized and observed. He took issue with Macaulay's praise of superficial knowledge, drawing a distinction between knowledge and wisdom. The latter marked off, in Forbes's view, the 'profound' engineer or lawyer. One gets the impression that Forbes felt that to push Watt's original scientific pretensions too far was to paint him as superficial and, in fact, to detract from his reputation as a profound engineer. Watt's champion in the water question, Henry Brougham, had earlier been nominated by Forbes as the very type of the superficial man of knowledge:

> I cannot help fancying that the character of Lord Brougham is a perfect index to the grand distinction between Knowledge & Wisdom. Great & quick abilities – an unbounded literary appetite and memory, & probably a real taste for Science; a powerful constitution & latterly much leisure, have altogether failed to make his Lordship's head anything better than a vast storehouse of knowledge; but which has produced nothing great of its own. For I suppose it is allowed on all hands that he is profound neither as a Lawyer, a Statesman or a Man of Science. But he must be the very beau-ideal of Macaulay's 19th Century man.[9]

Watt was different. He was a profound engineer. He was also a respectable, but not a profound, chemist, since in that department he had not produced anything great of his own. In accord with Wilson's advice, the section of Forbes's 'Preliminary Dissertation' dealing with Watt's work made no mention of the composition of water.[10] Forbes contrived instead to deal with the water controversy in the section dealing with the work of Cavendish.[11] He made a number of points. First, he sought to put the water question into perspective by arguing that, despite the long and

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[8] George Wilson to J.D. Forbes, 15 October 1855, Forbes Papers, Incoming Letters, 1855, no. 126.

[9] Forbes to Whewell, 29 October 1848, Whewell Papers, Add. Ms.a.204^{84}. For related correspondence on their respective responses to Macaulay see Forbes to Whewell, 19 October 1848, Add. Ms.a.$204^{83(1)}$, 15 November 1848, Add. Ms.a.204^{85}, and Whewell's letters to Forbes on this matter in Todhunter, *William Whewell*, 1876, vol. 2, pp. 346–50. Whewell and Forbes were reacting to a lecture given by Macaulay to the Edinburgh Literary Association in October 1848. For this episode as part of Whewell's and Forbes's concern to manage the forum of public debate about science, see Simon Schaffer, 'The history and geography of the intellectual world: Whewell's politics of language', in Menachem Fisch and Simon Schaffer (eds), *William Whewell: A Composite Portrait*, 1991, pp. 217–18.

[10] James David Forbes, 'Dissertation Sixth: Exhibiting a General View of the Progress of Mathematical and Physical Science principally from 1775 to 1850', *Encyclopaedia Britannica*, eighth edn, vol. 1, pp. 795–996.

[11] Forbes also apologized for inserting a biography of Cavendish 'into a chapter professedly on Heat … ' (p. 928, n 4). The section began with Forbes's version of the by now standard accounts of Cavendish's character: neither his nobility nor his wealth 'could withdraw him even for an hour from the course of study which he had marked out; and which constituted for him at once labour and relaxation, the end of living, and almost life itself' (pp. 928–29).

bitter controversy of recent times, the dispute 'hardly could be said to exist until the contemporary generation who witnessed the facts, and also the succeeding one had passed away'. Forbes contended that Cavendish until his death in 1810 and 'for nearly thirty years after' had the 'unquestioned tribute of at least the primary merit in so great a step in science'. Declining to analyse in detail a controversy 'purely personal, and which has almost filled volumes', Forbes proceeded to offer two considerations that he argued resolved the matter, one based on the behaviour of Watt, the other on the character of Cavendish.

Watt's behaviour in withdrawing his first letter to Priestley was not, Forbes stated, the behaviour of a man convinced of his ideas. Watt would not have revived his claim in public after this withdrawal 'had not the experiments and claims of Cavendish at home, and of Lavoisier in France, reanimated all his zeal for the assertion of his opinion'. Forbes doubted that we should allow Watt the advantage of 'anticipating the date of his matured conviction'. That is, we should not grant as a discovery at the time of its suppression a claim that Watt sought to suppress. Forbes also emphasized Watt's subsequent acceptance that Cavendish had a superior claim:

> Watt, in after life, may be said to have tacitly relinquished to Cavendish the honour which, in the first irritation of the conflict of their claims, he showed no disposition to do; it is, therefore, reasonable to infer that, on reflection, he saw good reasons for doing so. By this I mean that he suffered judgment to be passed in favour of Cavendish's claim in the writings of many of his eminent contemporaries, without attempting publicly to correct the all but universal impression which they made.[12]

Forbes believed that this argument (one also made by Harcourt) was clinched by Watt's behaviour in regard to Robison's article on 'Steam' in the 1797, third edition of the *Britannica*. When Watt edited and revised this he did nothing about a statement that it contained clearly attributing the discovery of the composition of water to Cavendish. Instead he 'permitted the fact to be thus transmitted to posterity'. Given this, Forbes believed, 'Watt's friends should have left the matter as he was content to leave it'.[13] Forbes was inclined to link the justice of attributing a discovery to a person with the degree to which they had worked and fought to lay claim to it. If they did not do that work, then, he argued, others should not take up the burden for them.

Having thus dealt with Watt's behaviour, Forbes turned, much more briefly, to Cavendish's character:

> it would yet be difficult to find in the whole range of scientific history (without excepting the venerable name of Newton), an individual so devoted to knowledge for its own sake, so indifferent to the rewards of discovery, so averse to the publication of what he felt to be important, and knew to be original, so insensible to the voice of praise when applied

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[12] Forbes, 'Dissertation Sixth', p. 930.

[13] Ibid. Watt Jr, of course, would have disagreed with Forbes's assessment of his father's attitude. He stated quite clearly that Watt continued to maintain his claim but would not take steps to deal publicly with contrary statements because of his confidence that posterity would judge the facts of the matter in his favour. Watt's supporters took his failure to intervene as a positive sign of his modesty and his lack of concern about fame in his own lifetime. (See Chapter 5.)

> to himself, so ardent in acquainting himself with the labours of others, and so liberal in assisting them.[14]

Forbes found it incredible that Cavendish would ever 'stoop even to the artifices of little minds for exalting his own reputation at the expense of others … '. From the perspective of the Watt camp there is something contradictory in this argument in that Watt's modesty is a ground for denying him the credit while Cavendish's modesty becomes the ground for granting it! Be this as it may, there was the argument in a nutshell. Forbes was aware, of course, of Wilson's reams of complex and convoluted argument and analysis exploring every aspect of the water controversy. In the end, however, the literate and even the scientific public that consulted the *Britannica* for its information needed only this argument from behaviour and character, needed only these indicators of truth. It is certain, given the limited circulation of Wilson's *Life* and the extensive circulation of the *Britannica*, that the attribution of the discovery to Cavendish was clinched in many more minds by Forbes's approach than by Wilson's.

The ninth edition of the *Britannica* contained new articles on 'Chemistry', 'Cavendish', 'Watt' and 'Water'. 'Chemistry' was contributed by Professor Henry Armstrong, Raphael Meldola and F.H. Butler. It contained no mention of Watt at all. The discussion of Cavendish's work described him as having 'discovered in 1781 that hydrogen and dephlogisticated air (oxygen), when exploded in a close vessel … produced pure water … Cavendish's discovery deprived it [water] of the rank of an element … and thus prepared the way for the acceptation of correct and definite views concerning the elementary bodies.'[15]

The article 'Cavendish' was an edited version of that in the eighth edition. It continued to describe the 1784 paper 'Experiments on air' as containing 'an account of two of the greatest discoveries that have ever been made in chemistry, – the composition of water, and that of nitric acid'. Although the work on composition of water was briefly described, interestingly, no effort was made to date the discovery as occurring in 1781. The usual depictions of Cavendish's character were provided and a new passage expressed his foundational importance in the history of chemistry: 'The splendid career of chemical investigation, which has since been pursued with a degree of success unprecedented in history, may be said to have been first laid open to mankind by his labours.'[16] Amidst all the enduring tropes about Cavendish's eccentricities this stands out as a key statement about the man. Cavendish was attributed that significance partly because of what he was taken to have discovered, but mainly because of the way that he discovered it. The rigorous, quantitative, extended train of research was the hallmark of Cavendish's work. It was the basis on which T.E. Thorpe designated him as the founder of modern chemistry.

When we turn to the entirely new article on 'James Watt' by Professor James A. Ewing, we do find, finally, some reference to the composition of water in the small print of the article. The small print mentions first some of Watt's minor innovations and then informs us:

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[14] Forbes, 'Dissertation Sixth', p. 930.

[15] 'Chemistry', *Encyclopaedia Britannica*, ninth edn, vol. 5, 1876, pp. 459–579, at p. 462.

[16] 'Cavendish, Henry', ibid., pp. 271–72, at p. 272.

> In the domain of pure science Watt claims recognition not only as having had ideas greatly in advance of his age regarding what is now called energy, but as a discoverer of the composition of water. Writing to Priestley in April 1783, with reference to some of Priestley's experiments, he suggests the theory that 'water is composed of dephlogisticated air and phlogiston deprived of part of their latent or elementary heat'. It is difficult to determine the exact meaning attached to these antiquated terms, and to say how far Watt's suggestion anticipated the fuller discovery of Cavendish.[17]

In this way the engineer's account of Watt, though going so far as to mention the water question, simply opted out of it. Watt was *a* discoverer but one with impenetrable and antiquated ideas. No such trouble was hinted at so far as the 'fuller discovery' of Cavendish was concerned, though, as we have seen, if Cavendish had been placed in context, this too could have been made out as antiquated. So far as the ninth edition of the *Britannica* was concerned overall, the place of Watt in the discovery of the composition of water was a historical curiosity and no more. Indeed, even the curiosity was limited!

Apart from the *Britannica*, another encyclopaedia of some consequence in the scientific world was the *Encyclopaedia Metropolitana*. This is best known to historians of science as the repository of two major treatises by John Herschel. Herschel wrote the almost 250-page treatise on 'Light' and also the one on 'Sound'. The former in particular was at the cutting edge of scientific work in 1828 when it was published and was an important document in the ongoing debate about the wave theory of light.[18] The articles 'Chemistry' and 'Electricity' for the *Metropolitana*, each of over 150 pages, were written by Herschel's Cambridge friend, the Reverend Francis Lunn.[19]

The main section on Cavendish in Lunn's 'Chemistry' recounted his major work in this fashion:

> The more prominent facts which were brought to light by the high talents of Mr Cavendish, most assiduously, yet cautiously exerted, were the compleat knowledge of hydrogen gas, which, though the substance had been obtained before, was quite disregarded until his time. These researches led him to the brilliant discovery of the composition of water, which he laid before the Royal Society in 1784 (Vide ELECTRICITY. Art 167). Thus was the synthesis of water accomplished.[20]

This statement conveys Cavendish's talent, his 'brilliant discovery' in unequivocal terms. There is no mention of Watt at all. If, however, we follow the advice to see the article 'Electricity', also written by Lunn, then we find a more detailed and extensive account incorporated into the section on electrolytic decomposition and

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[17] 'Watt, James', ibid., vol. 24, 1888, pp. 412–14, at p. 414.

[18] Richard Yeo, *Encyclopaedic Visions*, pp. 274–75.

[19] Francis Lunn (1795–1839) was a student with Herschel at St John's College, Cambridge. He was elected FRS in 1819 and was an early member of the Astronomical Society of London. He had assisted E.D. Clarke in experiments to isolate cadmium from zinc, and had been a candidate for the Professorship of Mineralogy at Cambridge in 1822. From 1828 to the end of his life he was Vicar of Butleigh in Somerset. (See Venn, *Alumni Cantabrigiensis*, Part I, vol. 4, p. 235.)

[20] Francis Lunn, 'Chemistry', *Encyclopaedia Metropolitana*, vol. 4, 1845, pp. 587–762, at p. 595.

composition of water. This in itself is interesting because it shows that by the 1830s and 1840s the water question, for contemporary chemists, was part of a rather different constellation. It had shifted from pneumatic chemistry to electrochemistry and had therefore been placed in a tradition that included later workers such as Davy, Faraday and, in the 1840s, William Grove, George Wilson and others. The statement made under 'Electricity' began with a clear attribution to Cavendish:

> In 1781, after some attempts made by Mr Warltire and Dr Priestley, who fired mixtures of common air and hydrogen in close vessels, and remarked an appearance of dew on the inner surfaces; the complete and satisfactory synthesis of water was performed by Mr Cavendish, to whom the honour of this discovery is generally and justly ascribed …[21]

The competitors were then mentioned but quickly sidelined. First Watt was dealt with:

> although it appears that Mr Watt, who had reasoned upon Priestley's experiments, had arrived at a similar conclusion, which he communicated to Dr Priestley by letter, dated April 26, 1783. Mr Cavendish, in 1781, burned 500,000 grain measures of hydrogen, and having collected 135 grams of pure water, ventured upon the bold conclusion, that water was composed of the two gases, oxygen and hydrogen.[22]

For someone aware of the detailed arguments bruited in the water controversy, even as this encyclopaedia article appeared, the rhetorical finesse of this statement would be apparent. First there is the impression given of timing: Cavendish's deeds were in 1781, Watt's in 1783. Cavendish's 'bold conclusion' is located in 1781, Watt's 'reasoning' in 1783. Then there is the matter of what they arrived at: Watt reached a 'similar conclusion'; Cavendish reached the modern view. Moreover he did so in a quantitative fashion. The impression conveyed is that Watt failed to quite get there, whereas Cavendish hit the bull's eye. Finally Lavoisier was dealt with: 'The celebrated Lavoisier having had a different object in view, though he made many experiments on these substances, did not arrive at the true composition of water, until he was informed by Sir Charles Blagden of Mr Cavendish's result, which he immediately verified on a larger scale.'[23] Lavoisier was reduced to verifying that which Cavendish had discovered.

We have seen that other, more complex, more equivocal stories than that given here could be told. The evidence that Cavendish drew any conclusion in 1781 was indirect. Harcourt and Wilson had had to run long, elaborate arguments of a hermeneutic kind to make their cases for Cavendish's priority. This is not to say that the arguments were not good ones. However, once we know the need for those arguments, the short-cuts taken in briefer accounts become clear to us. As the statements move through different literary forms they become simplified (of necessity), and what was equivocal and uncertain becomes unequivocal and definite.

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21 Francis Lunn, 'Electricity', ibid., vol. 5, 1845, pp. 41–172, at p. 109.

22 Ibid., p. 109.

23 Ibid.

Whether such simplification is justified is, in the end, a matter of judgement. Lunn was among those who, by gaining access to the *Metropolitana*, could make his judgement count.[24]

The evolving stance on the water question taken in various articles and editions of the *Britannica* and the *Metropolitana* was a predictable one given the affiliations of those writing and editing them. However, what eventually found its way into an encyclopaedia could also be, and often was, a matter of accident or happenstance. One should not expect clean consistency in such matters.

The *Penny Cyclopaedia* is a case in point. Whereas the *Britannica* was directed under Napier and Traill primarily to a scientific and learned audience, with the 'ordinary reader' catered to as a second priority, the *Penny Cyclopaedia* was directed to the lower classes.[25] It was published by the Society for the Diffusion of Useful Knowledge (SDUK). The Chairman of the SDUK's committee and, of course, one of the organization's founders was Lord Brougham. However, the *Cyclopaedia* did not clearly represent views on the water controversy in line with Brougham's. Whether through accident, inattention or design, the article on 'Water' offered only a few lines on the water question. We are simply told that 'The subject of the discovery of the composition of water has lately excited considerable discussion; we are however of opinion that the claim of Mr Cavendish as the author of this great discovery, and which has been for some years assigned to him without dispute, is rightly so attributed.'[26]

Here Watt was not mentioned and Cavendish awarded the discovery. However, in the article 'Watt, James'[27] a different, rather odd, story was told. First it was noted that 'little can be said here' about Watt's share in the discovery. He was stated to have been working on the question at the same time as Cavendish and Lavoisier. The curious were then referred to Arago's *Eloge* and to Brougham's 'Historical Account' appended thereto. Finally, the following strange statement was offered:

> it may suffice to observe that the great and pressing claims of Watt's professional avocations, together with his modesty and retiring habits, may in a great measure account for any difficulty that may arise in tracing the progress and extent of his discoveries in this, by no means the least important of the many subjects to which he addressed his comprehensive mind.[28]

This statement under 'Watt' is, as a whole, clearly pro-Watt, unlike that under 'Water'. This is evident from the authorities quoted – Arago and Brougham, Muirhead and Watt Jr are referred to, all of whom take Watt's part. Yet the oddly non-committal air to the statement is puzzling. Why, if pro-Watt, does the author bring up the 'difficulty' referred to at the end? Reading Arago, Brougham, Muirhead and

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[24] See Stephen Hilgartner, 'The dominant view of popularization: Conceptual problems, political uses', *Social Studies of Science*, **20**, 1990, 519–39.

[25] Yeo, *Encyclopaedic Visions*, pp. 277–78.

[26] See 'Water', *The Penny Cyclopaedia*, vol. 26, 1843, p. 110.

[27] 'Watt, James', ibid., vol. 24, 1843, pp. 135–43.

[28] Ibid., pp. 135–43, at p. 141.

Watt Jr would supply plenty of evidence for Watt's claim. So where is the difficulty? Perhaps the writer believed that there was little direct published evidence of Watt's inquiries in this area, inquiries that he supposed or knew to be *more* extensive than indicated in public accounts. The point, then, was that Watt's business preoccupations, his modesty and retiring habits prevented him from laying a more substantial public paper trail of his discoveries in the area. Given Brougham's and the SDUK's predilection to broadcast the achievements of men of modest backgrounds, it is surprising that more was not made of Watt's claims. This reminds us that, given the exigencies of production of these accounts, we should not necessarily expect coherence, consistency or clarity in them.

An important, and popular, work of the later nineteenth century was *Chambers's Encyclopaedia*. In its early editions this work trumpeted itself as 'A Dictionary of Universal Knowledge for the People'. It subsequently dropped its democratic designation in the 1895 edition, being content simply with 'Universal Knowledge'. Coverage of the water question shows an interesting shift when we compare the articles on 'Watt', 'Cavendish', and 'Water' in successive editions.[29] Table 11.1 summarizes the key changes.

The 1895 revision of the article on Watt suggests that a conscious decision was made to depict him as an engineer rather than as a 'man of science'.[30] The 'header' shows this clearly in its removal of that very term and the substitution of a narrow designation. The account of the experimental basis of Watt's steam-engine improvements (which had been highlighted in earlier editions) gave way to a moment of engineering inspiration. In concert with this, Watt's wide-ranging interest in, and contributions to, the physical sciences were replaced by the account of his garret workshop at Heathfield. In all editions the issue of Watt and the composition of water was referred to the article 'Water'. The referral in the 1895 edition placed his claims to priority firmly in the past whilst earlier editions treated them as current. When we examine the articles on 'Water', we find that Watt's claim was subordinated to that of Cavendish in all cases. If anything, the 1895 edition is more generous to Watt, although it does clearly give the priority to Cavendish and once again it tends to encapsulate the question as a historical curiosity. The article on 'Cavendish' changed more decisively, shifting from acknowledgement that Watt had a claim to dismissal of that claim.

Other encyclopaedias of the later nineteenth century exhibited similar, rather perplexing, inconsistencies. *The National Encyclopaedia*, published in fourteen volumes in 1884, had no entry for Henry Cavendish. The article 'James Watt' did mention his scientific pursuits. It had this to say on water: 'Concerning Watt's share in the discovery of the composition of water, an investigation in which he, Cavendish, and Lavoisier were engaged about the same time, we must refer those who are curious to Arago's Life of, or Eloge upon Watt, and to the "Historical Account of

[29] 'Watt, James', *Chambers's Encyclopaedia*, vol. 10, 1868, pp. 105–106 and vol. 10, 1895, pp. 578–79; 'Cavendish, Henry', ibid., vol. 2, 1861, p. 696 and vol. 3, 1895, pp. 36–37; 'Water', ibid., vol. 10, 1868, pp. 84–88 and vol. 10, 1895, pp. 563–71.

[30] This was in line with other later nineteenth-century writings about Watt, including those of Samuel Smiles, already noted, which downplayed the 'philosopher' in the engineer and emphasized the craftsman and the hard work.

11.1 Comparison of articles in editions of *Chambers's Encyclopaedia*

Article	1860–68, 1874 & 1883 editions	1895 edition
Watt, James	'WATT, JAMES, mechanician, engineer, and man of science … '	'WATT, JAMES, improver, and almost inventor, of the modern steam engine …'
	Contain extensive accounts of Watt's steam experiments as leading to separate condenser idea.	Account of steam experiments removed and replaced by simple statement: 'he hit upon the expedient of the separate condenser'.
	'He had a most extensive and accurate knowledge of the physical sciences, to several of which he made important contributions, and an almost unsurpassed fund of general information.'	Reference to physical sciences dropped and replaced by: 'The attic room at Heathfield Hall … where he used to work alone, is still preserved … Here he was perfectly happy working with his turning-lathe, and amongst his tools and models … He had a quickness of apprehension, a powerful memory, and an immense store of well-digested miscellaneous information outside his own domain … Watt stands at the head of all inventors … '.
	'His claims to be considered the discoverer of the composition of water are considered in the article WATER.'	'Watt's claims to be the first discoverer of the composition of water were long and strenuously maintained (see WATER, p. 565)'.
Cavendish	'he ascertained that water resulted from the union of two gases – a discovery, however, to which Watt (q.v.) is supposed to have an equal claim'.	'he ascertained that water resulted from the union of two gases – a discovery which has erroneously been claimed for Watt (q.v., see also WATER)'.
Water	Substantial section gives details of Cavendish's and Lavoisier's work and a history of the nineteenth-century controversy in some detail. At the end of this section: 'As we have no space to discuss Watt's real claims, we may here state that Dr George Wilson, whose *Life of Cavendish* is in reality a strictly impartial history of the water controversy, maintains on very solid grounds that in reality Watt was informed of Cavendish's discovery through Priestley, as Lavoisier was through Blagden.'	'The question as to who was the discoverer of the composition of water – the great Water Question – takes rank in the history of chemistry as the controversy as to the discovery of the calculus and of the planet Neptune in other sciences. Brougham, Brewster, Kopp, Arago, Dumas, and many others have maintained one or other of the theses; and the claims of Cavendish, James Watt, Priestley, and Lavoisier have been canvassed and defended. Research seems inclined to give the priority to Cavendish, while allowing that Watt made independent experiments and came to similar results soon after.'

the Discovery of the Composition of Water" by Lord Brougham.'[31] Obviously, the reader following this referral would be exposed to a strongly pro-Watt account. The entry on 'Water', however, told a very different story:

> Cavendish, by a series of ingenious experiments in the year 1781, demonstrated its [water's] chemical composition. He came to the conclusion that water consisted of dephlogisticated air (or oxygen) united with phlogiston (or hydrogen), and a similar conclusion was arrived at by James Watt, and by the French chemist Lavoisier. The attention of the former was directed to the subject by the account of Cavendish's experiments, which he received from Dr. Priestley, and the latter was stimulated by a similar account derived from Blagden. Attempts have most ungenerously been made to deprive Cavendish of the honour of the discovery, and fix it either upon the Frenchman or the Scotchman. But Dr. George Wilson, in his 'Life of Cavendish', and Dr. Whewell, in his 'Inductive Sciences', have successfully controverted these claims, and proved that all the credit should fairly be given to the great English philosopher.[32]

This time the authorities cited are strongly pro-Cavendish and the claims for Watt (and for Lavoisier) seen as ungenerous.

As a final example, consider *The Globe Encyclopaedia of Universal Information*, which was edited by John M. Ross and published in Edinburgh in the late 1870s. The article 'Chemistry' nominated Cavendish as the discoverer, as did the article on the great man himself. Neither so much as mentioned Watt. However, the article on Watt informed readers that Watt was 'quite at home' in the more theoretical departments of science and was 'an independent discoverer' of the composition of water, though Cavendish had the priority. The article 'Water' also had Cavendish proving its composition in 1781, but noted that Watt had a claim and that a bitter controversy had occurred. In the end, however, the priority was given to Cavendish, though Watt was recognized as an independent researcher on the topic.[33]

We can conclude that in the more erudite encyclopaedias and many of the popular ones, the standard stories in the later nineteenth century gave priority to Cavendish though retaining in some cases reference to the claims of Watt. Whatever lessons were learned about the water question by generations of students and general readers in the late nineteenth century, one can say that these lessons were neither entirely consistent nor uniform. In some senses, then, as a matter of 'public opinion' the water controversy remained partially open in the encyclopaedic realm, though overall the balance of credit certainly lay with Cavendish. In many cases the character of relevant articles might well owe much to 'scissors and paste' treatments of prior publications, but there is some evidence, especially in

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31 'Watt, James', *The National Encyclopaedia. A Dictionary of Universal Knowledge*, 1884, vol. 14, pp. 370–71, at p. 371.

32 'Water', ibid., pp. 361–63, at p. 361.

33 *The Globe Encyclopaedia of Universal Information*, 6 vols, 1877–79. See 'Cavendish, Henry', vol. 2; 'Chemistry', vol. 2, pp. 109–14, at p. 109; 'Water', vol. 6, pp. 476–77, at p. 477; 'Watt, James', vol. 6, p. 482. Another encyclopaedia consulted was *The Oracle Encyclopaedia. Profusely Illustrated. Containing the most Accurate Information in the most readable form*, published in 5 vols in London by George Newnes Ltd in 1895 and edited by R.W. Egerton Eastwick B.A. of the Middle Temple. The articles on Cavendish, Water and Watt were taken verbatim from *The Globe Encyclopaedia*.

the case of the eighth edition of the *Britannica* and *Chambers's Encyclopaedia*, for conscious crafting.

We now turn to a more extensive body of literature which also played an important part in fixing opinion on the 'water question' – chemical textbooks.

The Water Question in Textbooks of Chemistry

The relationship between textbooks and history is an interesting and important one. Historical accounts of scientific work can be used as introductions to the current state of play. Priestley's writings would be an example. History can also be used consciously as a pedagogic device or to serve didactic or nationalistic purposes.[34] But more frequently, perhaps, history is used simply in the 'who, what and when' mode in which a brief, obligatory mention is made of the first person to do something. This process is part of the process for allocating credit in science. Through it succeeding generations of students pay homage to their scientific forebears and students and practitioners of a science acquire basic historical navigational beacons. There is usually no room or desire in such accounts for long, complex or equivocal historical disquisitions. Thus, whatever the state of play in a contested allocation of credit, the author of the text will feel pressure to close that contest for himself and his readers.[35] Because of this, a popular, widely employed text can be enormously influential among large bodies of people, almost silently resolving controversies and contested points that may persist in more arcane forums less directly relevant to disciplinary practice.

Chemical texts of all kinds proliferated in the later nineteenth century as the teaching of chemistry in universities, colleges and schools expanded enormously. My concern is to gain a sense of how the water question was dealt with in this literature. In so far as the literature dealt with the question at all it ought perhaps to reflect sentiment within the chemical community. Equally the ideas propagated concerning the water question would themselves bear, in ways and to an extent perhaps hard to determine, upon the career of the controversy itself. A survey of this literature is, then, in the nature of a test of the hypothesis that the views of the controversy in expert circles eventually found their way to other 'levels'.

I have examined a large sample of texts and the summary results of that examination are presented in the Appendix. The sample consists of chemical texts with 'chemistry' in the title listed in the main catalogues of the National Library of Scotland and the Edinburgh University Library, and published between 1830 and 1900. To make the task more manageable, I screened out specialist works and texts intended for particular groups of students (such as medical students), or dealing with specialist aspects of

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[34] See Colin A. Russell, '"Rude and Disgraceful Beginnings": A view of history of chemistry from the nineteenth century', *The British Journal for the History of Science*, **21**, 1988, 273–94. Part of Russell's aim in this paper is to question some of the knee-jerk dismissals of the work of chemist–historians of the nineteenth century.

[35] This is a line of thought no doubt inspired by Latour and Woolgar's discussion of statement types and the addition and removal of 'modalities'. See Latour and Woolgar, *Laboratory Life*, 1979, pp. 81–86.

chemistry, including those dealing exclusively with organic chemistry. The remaining works I have examined for statements of relevance to the water controversy. 'Influential' works that merit more detailed attention can be identified by their multi-edition status. Some were popular because they were texts certified in some way. Thus, for example, in 1859 there were twenty-eight chemistry texts on the recommended list of the Department of Science and Art because they were regarded as particularly suitable for use in schools.[36] There exist other sorts of recommendations. For example, in 1886, William A. Tilden, Professor of Chemistry at the Mason College of Science in Birmingham, produced a set of desiderata for the Birmingham Reference Library.[37] For 'the science of chemistry as it now is' Tilden recommended as larger textbooks Roscoe & Schorlemmer, Miller, Fownes, and Bloxam. These all covered both experimental and theoretical chemistry. He recommended Hofmann's *Modern Chemistry*, Wurtz's *Atomic Theory*, and Remsen's *Principles of Modern Chemistry* as elementary works in theoretical chemistry. He suggested M.M. Pattison Muir's *Principles of Chemistry* as perhaps more for the teacher than for the student and identified Lothar Meyer's *Die Modernen Theorien der Chemie* as the most important general treatise. He also referred to Ostwald's *Lehrbücher*. In suggesting model original works that students should read he included Dumas's work on the composition of water in the *Annales de Chimie* of 1843. Thus as well as conducting an overall survey of texts and their attributional tendencies, it is also possible to isolate smaller samples of 'influential' works so identified in various ways.

My survey of chemical texts does reveal certain obvious patterns that should be apparent on perusal of the tabulated information in the Appendix. First, it must be said that a significant proportion of elementary chemical textbooks showed little concern with attribution, let alone history. Of ninety post-1840 texts surveyed, thirty-nine made no attribution for the discovery of the composition of water in their accounts of its synthesis and analysis. In the style of catechisms or recitations of facts and principles, they stuck strictly to the chemical subject matter. They named hardly any names. There was no concern to attach the facts presented to any particular discoverer. Understandably, the overwhelming concern was to convey the facts about the composition and properties of water rather than to even hint at the water question.

While this seems disproportionately true of elementary texts, it is also true of many more advanced ones. Large, systematic texts such as D.B. Reid's *Elements of Chemistry, Theoretical and Practical* adopted a strict 'universal' form. The section on water reads thus:

> SECT.1. – WATER
> Symb. H; Eq by W., 9; by volume □ (one measure). Specific gravity, 1.000. One cubic inch weighs 252.458 grains at 62° Fahr., Bar. 30°; it is about 815 times heavier than air,

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[36] See James Tilleard, *On Elementary School Books*, 1860, p. 4. (This work was reprinted from the *Transactions of the National Association for the Promotion of Social Science*, 1859.)

[37] William A. Tilden, *Books on Chemistry. Birmingham Reference Library Lectures*, 1886. It is worth noting that Tilden prefaced his remarks on texts by a brief survey of the history of chemistry. Noting key advances that had established chemistry as a science, he included 'The examination of hydrogen (1766) and the experimental proofs of the composition of water (1781) both due to Cavendish … '. He gave Wilson's *Life of Cavendish* as his reference.

> and when converted into vapour at 212°, it expands to 1696 times the volume it occupies at its greatest density. The specific gravity of steam (air at 212° being 1) is 0.625, or 0.484; air at 60° = 1 …[38]

And when the explosion experiment is described it is again in very neutral terms:

> When a mixture of half a measure of oxygen gas (□) with one measure of hydrogen (□) is inflamed in a dry glass vessel, both gases entirely disappear, and the interior surface of the vessel is found bedewed with moisture, formed by the condensation of the watery vapour that results from the combination.[39]

This text would have been widely used by Reid's students at the Edinburgh School of Arts and in his practical chemistry classes at the University of Edinburgh.

William Gregory's *Outlines of Chemistry, For the Use of Students* was designed for his own students at the University of Edinburgh and for similar groups. In explaining the form of his work Gregory emphasized, in 1845, the crowding of the curriculum and took the historic step, especially for a Professor of Chemistry at Edinburgh in direct line from Black, of removing any treatment of the 'Imponderables', that is, Heat, Light, Electricity and Magnetism. These belonged to physics, he said, but the main reason for leaving them out was 'because the enormously increased extent and importance of Chemistry, especially of Organic Chemistry, rendered every moment of time, in a course of lectures … precious in the highest degree'.[40] Gregory was the man who completely rewrote the article 'Chemistry' for the eighth edition of the *Encyclopaedia Britannica* in an entirely systematic way, removing virtually all historical treatment, and, in the process, attributions of discoveries. For a character such as Gregory, the pressures of pedagogy decisively crowded out attributions, of which there are hardly any in his book, and none concerning the composition of water.

Works that responded to the escalation of possible content rather differently also often had a different attitude to 'extraneous' attributional and historical material. Albert Bernays's *First Lines in Chemistry* is an example. In justifying his text, Bernays explained that he had 'vainly sought a manual for his pupils with a clear and simple exposition of the leading facts and principles of the science, unencumbered by a mass of information, of no service except to advanced students'.

> The plan adopted by most manuals of Chemistry in our language, of discussing in a long introduction, which is never read, the most recondite laws of the science, and the most intricate facts, before the student has mastered the simplest, is one that I have never been able to see the advantage of …[41]

It seems probable that his selectivity and thoughtful pedagogy was linked to Bernays's decision to include a good deal of modest historical information throughout

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[38] D.B. Reid, *Elements of Chemistry, Theoretical and Practical*, 3rd edn, 1839, p. 31.
[39] Ibid., p. 99.
[40] William Gregory, *Outlines of Chemistry, For the Use of Students*, 2 vols, 1845.
[41] A.J. Bernays, *First Lines in Chemistry. A Manual for Students*, 1855, p. vii.

his text. Thus his discussion of 'Hydrogen' begins: 'Discovered by Paracelsus in the 16th century, but first properly investigated by the illustrious Henry Cavendish in 1781. It is the base of water … '.[42] The treatment of water positively overflowed with historical information:

> Water was long considered as an elementary substance; it was even supposed capable of being converted into earth, till Lavoisier proved the earth to be derived from the vessels in which the operation was conducted. The composition of water was first demonstrated by Cavendish in 1781, and almost about [*sic*] the same time by Watt, in so far as both proved that water was always formed when hydrogen was inflamed in oxygen. Subsequently Lavoisier decomposed water into its elements.[43]

Without more detailed research it is impossible to state categorically why Bernays's approach differed so substantially from Reid's. Besides his more selective and relaxed approach to pedagogy, we might point to the texts he relied upon and also to the tradition within which he trained. He had studied for his PhD under Liebig at Giessen, and it is noticeable that authors of texts who shared this experience also seemed sensitive to historical matters.[44]

Robert Dundas Thomson was an example, going to Giessen in 1840 after having trained for the medical profession in Edinburgh and Glasgow, and having studied chemistry at Glasgow under his uncle, Thomas Thomson. He assisted his uncle in lecturing on chemistry at Glasgow University from 1841, but failed in a bid to succeed to the Chair of chemistry in 1852. In his *School Chemistry*, Thomson systematically included historical information in a rather telegraphic form. For example, under 'Hydrogen' we find:

> *History* (Mayow 1674) Inflammable air (Boyle, Hales and Cavendish before 1766) Phlogiston. Hydrogen … (Lavoisier, 1787) …
> *Preparation*. The source of hydrogen, in whatever manner it be prepared, is water, which is composed, as Watt suggested from Warltire and Priestley's experiments (1781) and as Cavendish demonstrated (1781), of oxygen and hydrogen.[45]

Although his uncle was regarded as an 'enemy' by the Watt camp, the nephew seems to have been unusually generous in his acknowledgement of Watt in a school text.

One of the most successful of nineteenth-century chemical texts was George Fownes, *A Manual of Elementary Chemistry. Theoretical and Practical*. First published in 1844, the work went through its eleventh edition within thirty years, helped, no doubt, by the favour it found with the Reverend Henry Moseley, Chief

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42 Ibid., p. 27.

43 Ibid., p. 33.

44 Bernays described his book as founded on 'the manuals of Berzelius, Gmelin, Regnault, Brande, Liebig and Turner, Woehler, Fownes, Gerhardt and others'. Bernays also wrote popular texts such as *Household Chemistry*, 1852, gave numerous public lectures, and was interested in 'social matters'. See *DNB*, **22**, Supplement, pp. 183–84.

45 Robert Dundas Thomson, *School Chemistry: or, Practical Rudiments of the Science*, 1848, pp. 47–48.

Inspector of Schools in the Education Office.[46] Fownes had studied under Professor Thomas Everitt at Middlesex Hospital and then undertaken a PhD at Giessen. He worked as an assistant to Thomas Graham in the laboratory at University College London, where his path would have crossed that of George Wilson, who was a student there at the same time. Fownes resigned in 1840 to become a lecturer on chemistry at Charing Cross Hospital. He became Professor of Chemistry to the Pharmaceutical Society in 1842 and in the same year succeeded Everitt at Middlesex Hospital. In 1845 he resigned from his hospital appointment because of illness but in 1846 took up the Professorship of Practical Chemistry in the Birkbeck Laboratory at University College. At the time of his death in 1849, he was Secretary of the Chemical Society.

The two editions of Fownes's *Manual of Elementary Chemistry* that were published during his lifetime (in 1844 and 1848) and the third edition of 1850, published shortly after his death, made an apparently quite unequivocal statement on the water question. Under 'Hydrogen' we find:

> It appears that the composition of water was first demonstrated in the year 1781 by Mr Cavendish, but the discovery of the exact proportions in which oxygen and hydrogen unite in generating that most important compound has from time to time to the present day occupied the attention of some of the most distinguished cultivators of chemical science.[47]

This is an interesting statement. Although making the attribution to Cavendish in a manner not surprising in an upwardly mobile young chemist serving as Secretary of the Chemical Society in the 1840s, it is carefully worded. Whilst the neophyte would probably take away from this simply the attribution of discovery to Cavendish, the informed reader would learn more, or perhaps we should say less, because the information would be more equivocal. The 'It appears' would signal knowledge that there was an issue here, that there was a water question. The 'first demonstrated' would indicate Fownes's awareness of key criteria of attribution lying in demonstration, as opposed to merely having an idea. The remarks about the composition of water continuing to engage some of the best chemists perhaps conveyed the idea that Cavendish's work, in this as in other areas, was still live and challenging.

By the seventh edition of 1858, with H. Bence Jones and A.W. Hofmann now editing the work, an interesting addition had occurred in the form of a footnote to the paragraph quoted above. After Cavendish's name an asterisk now led the reader to the following footnote:

> *A claim to the discovery of the composition of water, on behalf of Mr James Watt, has been very strongly urged, and supported by such evidence that the reader of the controversy may be led to the conclusions that the discovery was made by both parties, nearly simultaneously, and unbeknown to each other.[48]

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46 See David Layton, *Science for the People. The Origins of the School Science Curriculum in England*, 1973, pp. 99–100 on Moseley's emphasis on the importance of teaching chemistry in schools and his favoured texts.

47 George Fownes, *A Manual of Elementary Chemistry, Theoretical and Practical*, 1844, p. 105.

48 Fownes, *A Manual*, 7th edn, 1858, p. 123.

Again the reasons for this change involve speculation. Had the agitations of the 1840s finally become too much to ignore? Was one of the editors a Watt supporter? Whatever the case, this formulation remained unchanged until the tenth edition of 1868. There the following remark was added at the end of the above footnote: 'See the article "Gas" by Dr. Paul in Watts's *Dictionary of Chemistry,* ii, 780.' The keen student who followed up this apparently innocuous further reference would find a surprisingly long and complex essay on the water question. Dr Paul thoroughly contextualized both Watt's claim and Cavendish's 'discovery' and, on the basis that Cavendish's true meaning on the composition of water fell short of the mark, ended up giving the major credit to Lavoisier since Cavendish did not do 'anything more than supply the evidence of the composition of water'.[49]

Paul dealt decisively with any claim for Watt, stating that such a claim 'appears to involve a disregard of what really constitutes a title to be considered as a discoverer'.

> Watt certainly was the first to put forward, in 1783, the opinion that water was a compound of inflammable and dephlogisticated air; but that opinion was merely an hypothesis, based upon data furnished to him by Priestley, and unsupported by any experimental observation of his own. In fact, he never appears to have laid claim to anything more than having put forward this view as a speculation; and though, for a time, he believed he had been unfairly treated, his only complaint was that his 'ideas' had been pirated, and that no mention had been made of his 'theory', either by Cavendish or Lavoisier …[50]

Paul's remarkable article disappeared from the edition of *Watts' Dictionary of Chemistry* that was revised and entirely rewritten by Pattison Muir and Foster Morley in 1894. The article 'Gas' was removed, and discussion of the history of the composition of water was shifted back to the article 'Water'. Now, in a few column inches, Cavendish's archaic interpretation of his own experiments was subordinated to Lavoisier's accomplishment. Watt had disappeared.[51]

The career of the treatment of the water question through the many editions of Fownes's work and Watts's *Dictionary* is unusual. I have argued on the basis of the course of the 'live' controversy and treatments in major encyclopaedias that the categorical case for Cavendish was being established by the 1850s and 1860s. Yet

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49 B.H. P[aul], 'Gas', in Henry Watts (ed.), *A Dictionary of Chemistry and the Allied Branches of Other Sciences*, 1864, vol. 2, pp. 773–82, esp. pp. 779–82. Henry Watts had been involved with the editing of Fownes for some time. Dr Benjamin Horatio Paul (1827–1917) was trained in pharmacy, subsequently gaining his PhD with Leibig at Giessen in 1848, and also studying with Thomas Graham. He was elected FCS in 1868 and became editor of *The Pharmaceutical Journal* in 1870. He worked as a consulting chemist. See 'Benjamin Horatio Paul', *Journal of the Chemical Society*, **113–114**, 1918, 334–36.

50 Ibid., p. 782.

51 M.M. Pattison Muir and H. Foster Morley, *Watts' Dictionary of Chemistry. Revised and Entirely Rewritten*, 1894, vol. 4, pp. 859–60. The tone is captured in the following: 'Cavendish established the fact … but he stated this fact in language that no longer carries a definite meaning with it. Lavoisier added to the experimental basis whereon the fact rested, and he expressed the fact in language that still is clear, definite, and descriptive' (p. 860).

here we have an example where a categorical case (if a carefully worded one) was modified in the 1850s to give *more* credit to Watt. It was then further modified in the 1860s to pull the rug out from under *both* Watt and Cavendish in order to install Lavoisier as discoverer. Whatever the reason for this, through Fownes's *Manual* Watt's claim would have been kept alive to some degree for generations of chemistry students, even if, by the late 1860s, the more diligent of them who followed through to Dr Paul's article might end up as supporters of Lavoisier, as would readers of Watts's *Dictionary*!

Fownes was one of the texts recommended by Tilden in 1886. Another was Roscoe and Schorlemmer, a more advanced text, and one that contained the most extensive treatment of the water question, and of chemical history generally, of any textbook examined. Having stated in the Preface that their aim was 'to place before the reader a fairly complete, and yet a clear and succinct, statement of the facts of Modern Chemistry', Roscoe and Schorlemmer explained their use of history:

> The volume commences with a short historical sketch of the rise and progress of chemical science, and a few words relative to the history of each element and its more important compounds prefaces the systematic discussion of their chemical properties. For this portion of their work, the authors wish here to acknowledge their indebtedness to Hermann Kopp's classical works on the History of Chemistry.[52]

In their Historical Introduction, Roscoe and Schorlemmer depicted the discovery of the composition of water as being associated forever with the name of Cavendish. They argued that he was the first to actually set out to determine the nature of the water in the explosion experiments. They also stated that Cavendish was the first to show that the only product of the explosion of pure dephlogisticated with pure inflammable air is pure water. His accomplishment, as they put it, was to 'distinctly prove the fact of the composition of water'.[53] Beyond this, though, it appeared to them that Cavendish did not hold 'clear views' that water is a chemical compound of its two elementary constituents: 'On the contrary, he seems to have inclined to the opinion that the water formed was already contained in the inflammable air, notwithstanding the fact that in 1783 the celebrated James Watt had already expressed the opinion that "water is composed of dephlogisticated and inflammable air".'[54]

Their opinion on the controversy simply quoted Kopp to the effect that Cavendish 'first ascertained the facts' that were the basis of the discovery, whilst Watt was the 'first to argue from these facts concerning the compound nature of water', though he did not specify the components properly. Again, along with Kopp, they nominated Lavoisier as first clearly recognizing the compound nature of water and determining its components exactly.[55]

What happens in Roscoe and Schorlemmer when we turn from the nuanced Historical Introduction to the historical material presented in conjunction with the

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[52] H.E. Roscoe and C. Schorlemmer, *A Treatise on Chemistry. Volume 1. The Non-Metallic Elements*, 1877.

[53] Ibid., p. 21.

[54] Ibid.

[55] Ibid., pp. 30–31.

treatment of elements and compounds? Under 'Hydrogen' we find the following statement:

> It has already been stated (see Historical Introduction) that water was long supposed to be an elementary or simple substance, and it was not until the year 1781 that Cavendish proved that water was produced by the union of oxygen and hydrogen gases, whilst Humboldt and Gay-Lussac first showed in 1805 that these gases combine by volume in the simple relation of one to two.[56]

At this level, as expected, the statement is much more categorical and of the simple form 'in 1781 … Cavendish … proved', but the reader is referred to the Historical Introduction. The same is done in that portion of the text dealing with the 'Formation of Water':

> The question of the discovery of the composition of water, a substance which up to the last century was considered to be a simple body, has been fully discussed in the historical introduction. We there learned that Cavendish first ascertained that by the combustion of two volumes of hydrogen and one volume of oxygen, pure water and nothing else, is produced. Warped, however, as his mind was with the phlogistic theory, he did not fully understand these results, and the true explanation of the composition of water was first given by Lavoisier in 1783, when the French chemist repeated and confirmed the experiments of Cavendish. The apparatus, of much historical interest, used by him for proving that hydrogen gas is really contained in water is seen in facsimile in Fig. 60.[57]

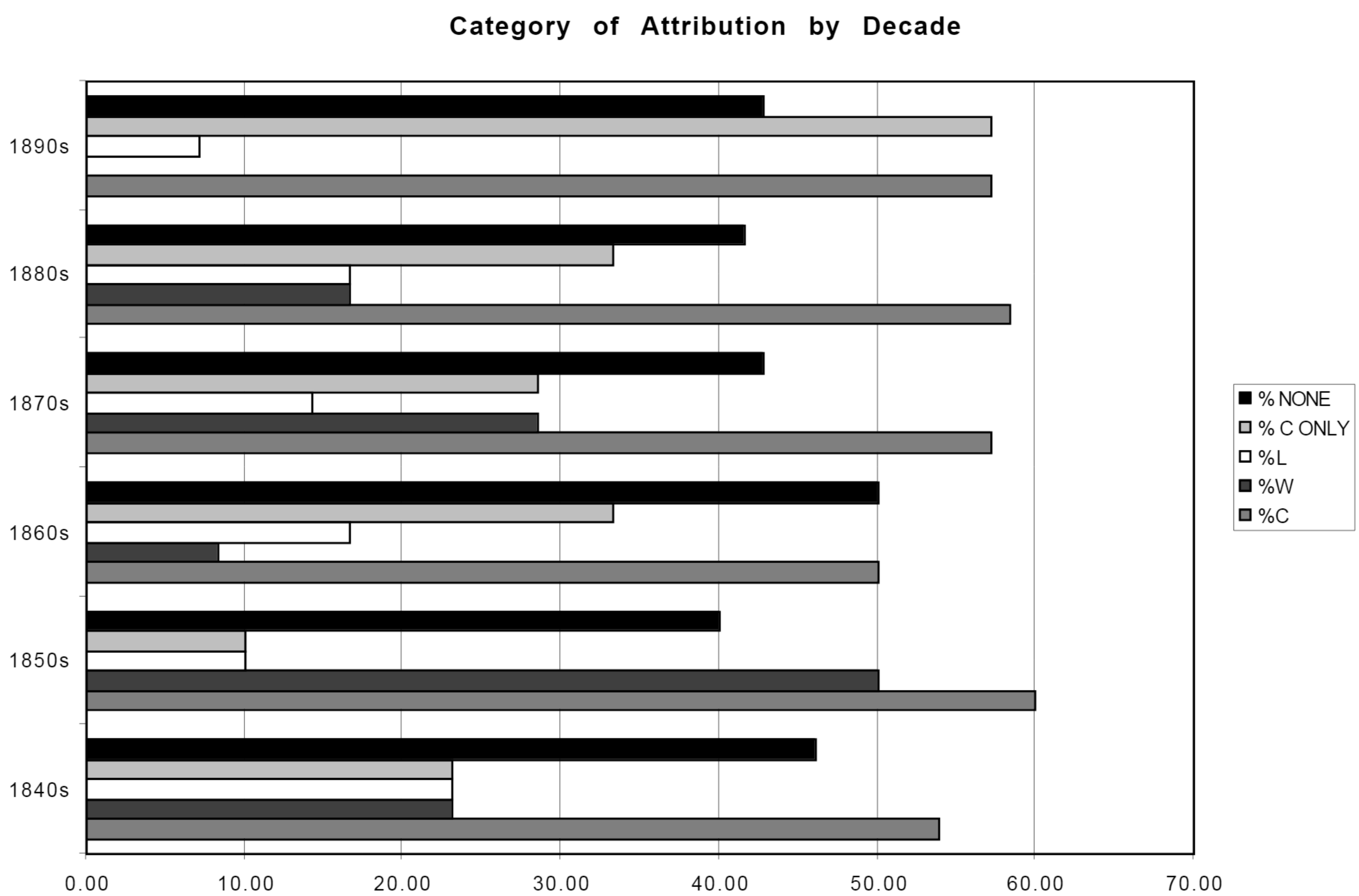
Notice, first, that in neither of the two extracts quoted above is Watt mentioned. At this level of historical information, one candidate for credit falls out of the picture. We seem to be left with Cavendish, at least to judge from the material under 'Hydrogen', but then, when we get to the formation of water, another twist occurs. Here the criteria are tightened and Cavendish's priority is presented as clouded by his archaic theoretical ideas. The 'true explanation' is now reserved for Lavoisier. It is not made clear whether the 'discovery' is to be regarded as his. This depends on how the term is to be used. It is notable that the term discovery is not linked to a particular person in either of these passages: Cavendish first 'proved' or 'ascertained' are the verbs used, not 'discovered'. Lavoisier too did not discover, but rather offered the 'true explanation'. It may well be that Roscoe and Schorlemmer were consciously and systematically avoiding attributions of discovery as such.

It has not been possible to explore in detail what was going on behind the attributions made in the texts highlighted above. Getting behind the texts would involve extensive consultation of manuscript materials outside the scope of the current project. However, whatever personal and institutional stories might be told about individual texts, there is a level at which the attributional process was 'genre

[56] Ibid., p. 99.

[57] Ibid., p. 202. Roscoe and Schorlemmer were among those authors of chemical texts who also wrote histories of chemistry. Thus: H.E. Roscoe, *John Dalton and the Rise of Modern Chemistry*, 1895; H.E. Roscoe and A. Harden, *A New View of the Origin of Dalton's Atomic Theory*, 1896; C. Schorlemmer, *The Rise and Development of Organic Chemistry*, 1879. Other prominent British examples included Thomas Thomson, M.M. Pattison Muir, William Tilden, T.E. Thorpe and W.A. Shenstone.

driven'. That is, the exigencies of chemical text composition and publishing inevitably limited the historical information that could be presented. This, I suggest, in itself favoured simple attributions to Cavendish despite the occasional availability of historically complex and subtle discussions such as those by B.H. Paul and by Roscoe and Schorlemmer. The necessities of simplification and condensation led to a large volume of chemical literature opting for a totally 'pared-down' view that Cavendish discovered the composition of water.[58] The common concern to present chemistry through the medium of experimental demonstration also clearly favoured Cavendish over Watt since the latter's work provided no clear experimental exemplar for pedagogic purposes.



11.1 Attributions of the discovery of the composition of water in a sample of chemistry textbooks, 1840–99

Figure 11.1 shows, for all texts in my sample between 1840 and 1899, the types of attributions made relating to the discovery of the composition of water. The attributions made in each category are indicated as a percentage of the total number

[58] The formulaic historical snippets included in many later nineteenth-century texts can be seen as the demise of a 'true' historical approach. Colin Russell cites Frankland and Japp's *Inorganic Chemistry*, which included a routine history section for all major compounds, as setting a pattern for generations of textbooks. Russell believes that this trend 'accelerated the dehistoricisation of chemistry'. See Colin A. Russell, '"Rude and Disgraceful Beginnings"', *The British Journal for the History of Science*, **21**, 1988, 285, 292.

of texts in each decade. Thus the top bar in each cluster indicates the percentage of texts that made no attribution, the second bar the percentage that attributed the discovery to Cavendish only, and so on. Given that some texts make attributions to more than one person, the percentages do not add to 100.

The following points are notable. Whilst the percentage of texts not making any attribution is reasonably constant, varying between 40 per cent and 50 per cent over the whole period, there is a clear tendency for attributions made to 'Cavendish only' to increase. By the 1890s almost 60 per cent of texts surveyed made the attribution to Cavendish alone. The variation in the percentage of attributions to Lavoisier is too small to read anything into it. We can say that Lavoisier did have a small following among historically conscious British chemists of the late nineteenth century, though their views were rarely if ever expressed in textbooks.[59] However, the variation in the figures for Watt are significant. We see clearly a rise in his fortunes in the 1850s but a decline in subsequent decades until, in the 1890s, none of the texts surveyed made an attribution of the discovery to him. It must be remembered that 100 per cent of texts making *any* attribution mentioned Cavendish; the only variability is in the extent to which others were mentioned also and in some cases given priority over him, or equality with him. Overall, the survey of chemistry textbooks clearly indicates that, through this medium, generations of students of chemistry imbibed a story of water in which Cavendish was the central figure.

Further Popular Attributions

There is a notable Scottish strand of literature giving the palm to Watt. William Anderson's *The Scottish Nation,* published in the early 1860s, described James Watt as 'a celebrated natural philosopher and civil engineer' and gave thanks to Arago for rightly establishing for Watt the credit for the 'greatest and most important discovery in modern chemistry, the discovery of the *components of water*'.[60] The entry also found it:

> Not less remarkable – and proof of the complacency of Watt's disposition – that beyond securing the reading of his paper and its insertion in the volume of the Journal referred to, – confiding in the justice of posterity, – he took no steps to vindicate the originality of his announcement even although urged to do so, than that so many years should have elapsed during which the merit of it has generally been assigned to others, until a foreign biographer, invoking that justice, has secured for him its recognition.[61]

[59] Apart from B.H. Paul, M.M. Pattison Muir is notable. See his *Heroes of Science. Chemists*, 1883, pp. 77–78, 92 and also Muir, *A History of Chemical Theories and Laws*, 1907, pp. 43–47, which described Cavendish as 'trammeled by the theory of phlogiston'. Lavoisier was 'the greatest of all chemists' in whom 'the power of destroying and the power of reconstructing are united in so extraordinary a degree' (p. 47).

[60] 'James Watt', in William Anderson, *The Scottish Nation*, 9 vols, 1882, vol. 9, pp. 613–22, at p. 620. This was first published in the early 1860s.

[61] Ibid., p. 620.

Unlike Forbes, who found Watt's modest behaviour a sign of his lack of seriousness in his claim, this author argued the opposite case.

It is well known that another famous Scot, Andrew Carnegie, wrote a biography of Watt (for the 'Famous Scots' series), which was published in 1905. Carnegie's treatment of the water question is remarkably decisive. The volume makes no mention of Cavendish at all even though the water question is dealt with in a number of places. At one point we are advised almost casually amidst an explanation of latent heat that 'water passed as an element until Watt found it was a compound'.[62] In another passage, the year 1783 was described as a good one for Watt because '[h]is celebrated discovery of the composition of water' was published then. Moreover, 'the attempts made to deprive him of the honour of making this discovery ended in complete failure. Sir Humphry Davy, Henry, Arago, Liebig, and many others of the highest authority acknowledged and established Watt's claims.'[63]

Yet another genre worth dipping into in order to see what was said about the water question is the literature of uplift so prevalent in the middle decades of the nineteenth century, much of it intended for young readers. Many examples of this genre emphasized the way in which success had been achieved in the face of great difficulty. An example of this was *The Pursuit of Knowledge under Difficulties* by G.L. Craik.[64] The work was first published by the SDUK in 1830 without Craik's name upon it. Both Watt and Cavendish received attention. The few pages on Cavendish appeared in the second of two chapters headed 'Advantages of Wealth', and also pursued the theme of Cavendish rising above the distractions that his wealth might have induced. The discovery of the composition of water was also atributed to Cavendish, and remarks were made about his 'cautious and scrutinizing observation by which alone truth is to be detected'. The chapter on Watt and the steam engine made no mention of the water question at all.

The new edition of *Pursuit*, published in revised and enlarged form in 1858, this time explicitly under Craik's authorship, included a number of crucial changes. The section on Cavendish still remarked upon the importance of his contributions to pneumatic chemistry, but continued 'if we may no longer assign to him the undisputed glory of the great discovery of the composition of water … he will certainly for ever be remembered as the chief author of the experimental investigation which led to it'. A long scholarly footnote, rather out of place in the work as a whole, recounted the saga of the water controversy and its various interventions. The claim for Watt was carefully circumscribed as 'merely the merit of having been the first to perceive the full import of Cavendish's decisive experiment … and his statement … is certainly the earliest on record'. It was further acknowledged that Watt had been gestating this idea for some time. But the limitations of Watt's experimental contribution were noted, as were the doubts entertained about the 'absolute correctness' of his theory. Also, Cavendish's independence of Watt in drawing his own conclusions was asserted. Nevertheless, overall in this case the controversy had ensured that previously straightforward attributions to Cavendish

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[62] Andrew Carnegie, *James Watt*, 1905, p. 36.

[63] Ibid., pp. 114–15.

[64] [G.L. Craik], *The Pursuit of Knowledge under Difficulties*, 2 vols, 1830 in the SDUK 'Library of Entertaining Knowledge'.

were now complicated and given more nuanced expression in at least some cases. Although I have no documentary evidence to support the idea, it does seem possible that the SDUK, the author Craik and the publisher Charles Knight may in some combination have been swayed to make these changes because of the intimate part their 'leader', Henry Brougham, played in the water controversy as a key member of the Watt camp.

Another volume, *Perseverence under Difficulties as shown in the Lives of Great Men*, published by the Society for Promoting Christian Knowledge in 1862, discussed similar issues. Having noted that Watt was self-taught in chemistry as in all else, the author stated that Watt's chemical reputation 'rests principally on an important discovery that he made – *the composition of water*'.

> Watt was the first person to discover that [water] was composed of two different kinds of gases, namely oxygen and hydrogen … The honour … was claimed by other people besides Watt, among whom were the eminent philosopher Mr Cavendish, and M. Lavoisier a great French chemist; and there were many disputes and controversies on the subject … After a careful comparison of dates it has now been proved for certain that though Mr Cavendish and others may have made a similar discovery a short time later, the fact that water was a compound of two gases was first found out by Watt … If Watt had had nothing to do with steam, his great scientific attainments, and this discovery especially, would justly entitle him to the character of a great natural philosopher and man of science.[65]

This, in pure form, is the kind of popular account of Watt as scientific discoverer that people such as Whewell and Forbes were intent upon stamping out. It is identifiably drawing upon the synonymity and priority argument of the Watt camp, admitting of no significant difference between Watt's views and those of Cavendish and Lavoisier. It also sees no problem in identifying Watt as a great natural philosopher and man of science. It is evidence that in popular forums Watt as discoverer proved remarkably long-lived. Ironically, however, this rump of popular support for Watt probably helped to secure the discovery of the composition of water as a natural event for which Cavendish was responsible. The transparently nationalist literature, certainly, provided a useful object lesson in how nationalist sentiment could distort 'true' appreciation of the facts about discovery. The historiography of the water controversy readily imbibed this asymmetry.

Conclusion

It must be said that, taken together, the career of accounts in encyclopaedias, and the overwhelming tendency of historical renderings proffered in textbooks, helped to close the water controversy as all but a historical curiosity. Although the claims of Watt were kept alive to a perhaps surprising extent in the learned and technical literature, and a minority of British chemists plumped for Lavoisier, claims for Cavendish dominated. Only in certain pockets of nationalistic and self-help literature

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65 *Perseverence under Difficulties as shown in the Lives of Great Men*, 1862, pp. 210–11.

was Watt the heroic discoverer of the nature of water kept alive into the new century. By the time of the Watt centenary celebrations in 1919 those charged with the commemoration of his life showed little or no interest in the water question. The official centenary volume, by Dickinson and Jenkins, is a massive work, but it contains only a few references to the water question and those are bibliographical ones.[66] The focus of the centenary meetings themselves was upon engineering and upon Boulton & Watt engines. Official parties attended a garden party at Heathfield House and were taken to view the carefully preserved workshop above the kitchen where Watt the craftsman had spent so many hours during his later years.[67] It was this Watt, the engineer and craftsman, not the philosopher and man of science, who emerged from the plethora of writings about him in the late nineteenth and early twentieth century.

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66 H.W. Dickinson and Rhys Jenkins, *James Watt and the Steam Engine. The Memorial Volume Prepared for the Committee of the Watt Centenary Commemoration at Birmingham 1919*, 1927, pp. 59, 361, 367, 370.

67 Ibid., pp. 401–404.

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Chapter 12

Conclusions

From a modern perspective one might question the need to take the water controversy at all seriously. The Watt case is usually thought to be rather weak. In terms of the scientific credibility of their supporters, Watt and Cavendish appear to the modern eye unequally matched in this delayed-priority dispute. Cavendish commanded the BAAS leadership and the emergent chemical élite. Watt, by contrast, had Watt Jr, Muirhead, Brougham, Jeffrey and latterly Brewster in his corner and also Arago, with more distant support from Dumas. The filial thrust and the pleadings of Watt's advocates lacked scientific credibility. Indeed, in adopting the empirical conception of discovery and seeking to shift the terms of the contest to approximate to a purely evidential matter, the advocates of Watt tacitly, and sometimes explicitly, acknowledged their lack of scientific credibility.

Perhaps we too should simply admit that the case for Watt was simply a short-lived exercise in 'hype' with little of substance going for it. Although I have declined in this work to give a full account of the first phase of the water controversy, the story told in Chapter 3 and in the first attributional survey does make a number of relevant points. First it indicates that when looked at in context, the contributions of both Watt and Cavendish were credible ones. There is no intrinsic reason why Watt should not have come out of that situation with recognition as a discoverer. However, the retrospective recasting of Cavendish's work, which began in the 1780s, had gone a long way by the 1830s. His work had been 'modernized' to accord strongly with the New Chemistry in ways that Cavendish himself had not intended or hardly accepted. Watt, partly because of his own lack of ongoing involvement in the chemical debates, attracted much less attention of this sort, so that his ideas were much easier to restore to their original context. For this reason, the Watt camp in the second phase of the controversy faced a steep interpretative gradient.

Watt's strength lay not just among his immediate supporters, who openly and publicly participated in the water controversy. It was also among a much wider constituency of 'labourers in the vineyards' and industrially linked middle class. There were various reasons why Muirhead's texts appealed to such characters. Watt's symbolic importance to ideas about industrialization, political economy and the patent question must not be forgotten as we explore the intriguing ins and outs of the water controversy. That controversy was sustained in part by its resonance with these other, larger questions. Watt's reputation was what linked them. If Watt could be claimed as a natural philosopher, then certain positions in those larger ideological struggles were easier to sustain. This was why Watt's claim to the discovery of the composition of water (and his claim to be independent of Black in his steam-engine improvements) assumed such importance. They were both vital to

his *independent* philosophical reputation. On the other hand, to diminish Watt's independent philosophical reputation, as the supporters of Cavendish did, both in the water controversy proper and in the matter of Watt's relations with Black, was to reassert distinctions between 'pure' and 'applied' science and the hierarchical scientific/technical order that those distinctions sustained.

Along the way we have seen various broader cultural contexts within which, and through which, the water controversy was given life. In the case of Arago it was French industrialization and the attempted democratization of scientific institutions, in the sense of making them more publicly accountable, that stimulated the presentation of Watt as a philosopher and an engineer of humble origins transforming his national economy. In the case of Brougham the fight for worker education, the promotion of useful knowledge and the issue of patent reform were all causes in which the philosophical Watt was an asset. The chemists were preoccupied with issues of disciplinary identity and character to which the competing icons of Cavendish and Watt were very important. The 'Gentlemen of Science' also were fighting other battles closely related to the 'water question' – seeking the dominance of their kind of science and preferred mode of scientific organization via debates on issues such as Newton's character and the controversy over the discovery of Neptune. Their dominion in British science in the 1840s and 1850s depended upon a distancing of scientific discovery from industrial application, upon a link between the two that was mediated, or at least acknowledged to be so mediated, by themselves and not others.[1]

The contest over discovery and its fundamental contestability has been the other chief concern of this study. Great confidence was exhibited by the actors in the second phase of the controversy (and has also been shown by historians) about the certainty with which the true discoverer could be identified. My stance has been to question this certainty, to show the inherent instability and contestability of empirical and more complex philosophical criteria of discovery. Whatever criterion of 'discovery' or 'discoverer' we might set up, it can always be shown to be wanting in a logical point of view. In fact neither reason nor 'nature' (in this case meaning historical action treated naturalistically) can operate as external determinants in the identification of discoverers. The case remains, however, that controversies such as the water question are conducted as if such external determinants do operate. I have seen it as my task to take the reader behind the discovery accounts of the historical actors in the controversy in order to lay out the rhetorical character of those accounts and to indicate the grounding of rhetorical strategy in circumstance. The arguments in the water controversy were interested arguments. They were made because more than 'the truth' was at stake. It mattered in a variety of ways whether Watt or Cavendish (or Lavoisier, for that matter) was identified as the discoverer of the composition of water.

My efforts to place the arguments of the protagonists in context have been driven by the desire to identify the interests at play in the story. The identification of

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[1] The publication of *Vestiges of the Natural History of Creation* was the other great challenge to the interpretative primacy of scientific elites in negotiating, in that case the theological, relations of science. On this see James A. Secord, *Victorian Sensation. The Extraordinary Publication, Reception and Secret Authorship of* Vestiges of the Natural History of Creation, 2000.

interests is itself a difficult exercise. Accounts that rely upon unidimensional and static interest stories are certainly unsatisfactory. In the case of the water controversy, nationalism has often been appealed to, the struggle between the supporters of Cavendish and of Watt being portrayed as driven by English versus Scottish nationalism. (The usual subtext of this sort of explanation had been that only such irrationalisms as nationalism could explain why the controversy kept going when it was obvious that Cavendish had the stronger claim.) There is no doubt that national feeling was an element in the story. There were junctures, mainly on the Watt side, for example, where individuals privately expressed or validated such sentiments. Watt Jr, for example, privately expressed the view that his father's case might benefit and should benefit from national sentiment. David Brewster, more than anyone, linked the controversy to national feeling by invoking what he regarded as a Cambridge conspiracy against all 'Scotchmen'. National feeling is also shown by the way in which attributions in popular culture in the late nineteenth century and subsequently include a strong national component in the case of Watt. The latter attributions are perhaps fairly pure instances of national feeling in action. But in the midst of the controversy, national feeling is not a good guide to allegiances. Brewster himself long supported the Cavendish case, albeit in his own distinctive fashion. George Wilson, his teacher Thomas Graham, James David Forbes and numerous other Scottish chemists and natural philosophers participated in the controversy on the side of the Englishman.

So national feeling as an interest is cut across, if not obliterated, in many cases by other interests and concerns. Notable here are what Barnes calls 'professional vested interests'. The state of chemistry in the 1840s in Britain as a field of inquiry, its social position and the aspirations of its practitioners, informed a set of professional vested interests. Those interests were in having chemistry seen, at the élite level, as a quantitative discipline and an abstract science not to be equated with the empirical methods of the practical chemist. Yet in creating such a distance from practical chemistry, the élite did not want to divorce themselves from practical outcomes. Far from it, they wished to portray their abstract chemical work as ultimately the basis of major practical innovations of economic importance. For characters such as Thomas Graham, George Wilson or Lyon Playfair, this sort of professional vested interest as a chemist cut across and ultimately overrode any nationalist sentiment in favour of Watt. For them, in displaying their field in public forums, the association with Cavendish was the more desirable. For them, Cavendish personified the careful, cautious, quantitative, exact and abstract science with which their careers and their institutions were tied up. Watt might well be admitted as a competent chemist in his own time. Wilson was happy, for example, to admit and propagate that idea. But Cavendish (suitably abstracted from *his* context) was treated as a chemist in advance of his times who prefigured the chemistry of the mid-nineteenth century and became, in Thorpe's hands, its founder.

The case of David Brewster provided an interesting test case for my approach. Here was a prominent natural philosopher who changed sides in the controversy. After arguing for many years for Cavendish, against his national inclinations, Brewster suddenly switched to the Watt side. He did this after the publication of the Watt *Correspondence* and used this as his pretext, behaving as if he had been converted by the 'new' documents. In fact the documents were not new to him at all

– he had seen them privately before and drawn the opposite conclusion. What had changed were his relations with the British scientific élite. Long-standing tensions with the Cambridge group, especially Whewell and Airy, came to a crisis point in the early 1840s when Brewster's optical research was finally effectively excluded from the *Philosophical Transactions* of the Royal Society of London. Brewster subsequently made little effort to publish in outlets controlled by the scientific elite. He diverted his publications to the *Proceedings* of the Royal Society of Edinburgh. Brewster opted for 'outsider' status. He appears to have decided, when the opportunity arose with the publication of the *Correspondence*, that he could best promote his objectives by supporting the Watt cause in the water controversy and linking Watt's 'suppression' to the ongoing failings, as he saw them, of British scientific institutions, notably the Royal Society of London.

The Cambridge men were not a uniform 'interest group'. Certainly Whewell and Peacock, the clerical branch, to which we might add the clerical but non-Cambridge Harcourt and the Cambridge but non-clerical Airy, were very strongly pro-Cavendish. They were willing to take up the fight actively. They also fought most fiercely for hierarchical control of the scientific community, engaged in strenuous boundary-work at the pure–applied science border, and promoted an idealist model of discovery. Charles Babbage, although he did not become conspicuously involved in the water controversy, was clearly pro-Watt, and this squares well with his radical views on questions of scientific organization and the relations between science and technology. John Herschel was, as in much else, in a position intermediate between Babbage and Whewell. He was inclined to emphasize the place of experimental proof in scientific work and as a criterion of discovery. He was less of a hard-liner than his clerical friends on science–technology hierarchies, and more inclined than them to distribute credit between Cavendish and Watt. The Cambridge men, then, did not form a phalanx. Though if forced to choose on the water question, all except possibly Babbage would put Cavendish's claim above Watt's, there was subtlety and variability in their stances. That variability reflects their demeanour within the politics of science at this time. The same can be said for James David Forbes, a man who admired James Watt as a scientifically informed engineer of great profundity but found the work of Cavendish to be in tune with his own specialized ambitions and activities.

The pro-Cavendish camp were not just defending Cavendish's claims; they were defending their own status as the arbiters of science in early Victorian Britain. They claimed the expertise to pronounce on scientific questions and on historical questions of discovery. In many ways the chief danger that Watt Jr, Brougham, Muirhead and the like represented was that they challenged that claim. They did so, as did Brewster after his conversion, with circumstantial arguments and a model of discovery that they contended were accessible to all reasonable men.

Watt Jr, Muirhead and Brougham made clear occasionally some of the deeper currents of their opposition to the Cavendish camp. Whewell, Harcourt and Peacock they regarded as part of a presumptuous gang of ambitious, corrupt, clerical *arrivistes* deferring to old aristocratic forms of governance in carving out their careers. The British Association was also much denigrated as involving a kind of whipping up of the scientific mob in a way dangerous for scientific credibility and commercial integrity.

By the time that live controversy was winding down in the late 1840s and 1850s it was possible for both sides to be confident that they had been victorious. Members of the Watt camp were confident that they had had the best of the argument in the reviews and had secured a good deal of popular support. The members of the Cavendish camp were convinced that they had demonstrated the scientific case, whatever popular sentiment might judge. In many ways, however, the victory was still in the making. It came through the attributional processes that we surveyed in Chapter 11. It is notable that the Cavendish camp changed their argumentative strategy in more public forums in the 1850s. Perhaps wisely, they no longer appealed to the complex chemical arguments that formed the basis, or detailed rationale, of their own convictions. Instead they used the argument from Cavendish's character or, in the case of the 'Brown discovery', a more empirical mode of argument. Crucial to the success of the Cavendish camp was their control of the training of students and of the popularization of science. Chemists educated in Britain in the 1840s and the 1850s were trained to revere Cavendish as a founder of their discipline. When they in their turn came to write textbooks and the like, Cavendish was securely and unproblematically identified as the discoverer of the composition of water. Through such texts and popular accounts in encyclopaedias, and increasingly through historical works, the Cavendish gospel was propagated. In these accounts, any sense of the rhetorical and interested character of arguments in the water controversy was lost, as they thoroughly naturalized Cavendish's 1781 experiments as the discovery event, and reified the great natural philosopher as the discoverer. Watt supporters were generally less well placed to influence students and wider audiences. In this way the day was won for Cavendish amongst expert and popular opinion. Although Watt's claims did not die, and were propagated in much biographical literature with a nationalist leaning, the victory was Cavendish's. So it was that I, along with thousands of other young twentieth-century students of chemistry, thought we were following in the footsteps of Cavendish alone when we set out to blow ourselves up trying to synthesize water. For us, of course, James Watt was an engineer.

Appendix

Attributional Survey Database

The listing gives attributions of discovery of the composition of water as made in chemistry textbooks of 1840 to 1900.

Author	Title	Edition	Date	C	W	L	C>W	C=W	W>C	L>C	C>L	None
				Attribution								
Hoblyn, Richard D.	*A Manual of Chemistry*		1841									x
Kane, Robert	*Elements of Chemistry*		1841									x
Turner, Edward	*Elements of Chemistry*	6	1842	x		x					x	
Baxter, W. Raleigh	*The Hand Book of Chemistry*		1843									x
Daniell, J.F.	*An Introduction to the Study of Chemical Philosophy*		1843	x								
Balmain, William H.	*Lessons on Chemistry*		1844									x
Fownes, George	*A Manual of Elementary Chemistry*	1	1844	x								
Gregory, William	*Outlines of Chemistry*		1845									x
Sparkes, George	*An Easy Introduction to Chemistry*		1846									x
Brown, J.C.	*Lectures on Chemistry*		1846	x	x	x		x		x		
Parkes, Samuel	*An Elementary Treatise on Chemistry*		1848	x								
Thomson, Robert Dundas	*School Chemistry*		1848	x	x		x					
Gmelin, Leopold	*Hand-Book of Chemistry*		1848	x	x	x						
White, W.	*Chemistry for Students*		1851									x
Reid, D.B.	*Rudiments of Chemistry*	4	1851									x
Regnault, M.V.	*Elements of Chemistry*		1852	x	x	x		x				
Thomson, Robert Dundas	*Cyclopaedia of Chemistry*		1854	x	x		x					
Bernays, Albert J.	*Household Chemistry*	3	1854	x	x			x				
Bernays, Albert J.	*First Lines of Chemistry*		1855	x	x		x					
Glover, Robert Mortimer	*A Manual of Chemistry*		1855									x
Fownes, George	*A Manual of Elementary Chemistry*	7	1858	x	x			x				
Buckmaster, J.C.	*The Elements of Inorganic Chemistry*		1858	x								
Bidlake, J.P.	*Text-Book of Elementary Chemistry*		1858									x
Miller, William Allen	*Elements of Chemistry Part II*	2	1860	x								
Odling, William	*A Manual of Chemistry*		1861	x								
Pope, George	*A Class Book of Rudimentary Chemistry*		1864									x
Williamson, Alexander W.	*Chemistry for Students*		1865									x
Hudson, Fearnside	*Inorganic Chemistry for Science Classes*		1865									x
Hofmann, A.W.	*Introduction to Modern Chemistry*		1865									x
Roscoe, H.E.	*Lessons in Elementary Chemistry*		1866	x								
Bloxam, Charles L.	*Chemistry Inorganic and Organic*		1867	x								

Fownes, George	*A Manual of Elementary Chemistry*	10	1868	x	x	x	x		x		
Naquet, A.	*Principles of Chemistry Founded on Modern Theories*		1868	x		x				x	
Barff, F.S.	*An Introduction to Scientific Chemistry*	2	1869								x
Gill, C. Haughton	*Chemistry for Schools*		1869								x
Kay-Shuttleworth, U.J.	*First Principles of Modern Chemistry*	2	1870	x							
Hart, H. Martyn	*Elementary Chemistry*		1870								x
Grundy, Cuthbert C.	*An Introduction to the Study of Chymistry*		1870								x
Rodwell, G.F.	*A Dictionary of Science*		1871	x	x	x		x			
Roscoe, H.E.	*Chemistry*		1872								x
Meldola, Raphael	*Elementary Inorganic Chemistry*		1873	x							
Rigg, Arthur	*An Easy Introduction to Chemistry*		1873								x
Jamieson, Thomas	*Inorganic Chemistry*		1874								x
Brown, Alex Crum	*Chemistry*		1875	x							
Valentin, William George	*Introduction to Inorganic Chemistry*	3	1876								x
Tilden, William A	*Introduction to the Study of Chemical Philosophy*		1876	x	x			x			
Roscoe, H.E. & Schorlemmer, C.	*A Treatise of Chemistry, Vol 1*		1877	x	x	x	x		x		
Thorpe, T.E.	*A Manual of Inorganic Chemistry. Vol 1*		1877	x							
Kemshead, W.B.	*Inorganic Chemistry*		1877	x	x			x			
Morris, David	*A Class-Book of Inorganic Chemistry*	2	1880	x	x		x				
Greville, H. Leicester	*The Student's Hand-Book of Chemistry*		1881	x							
Faulkner, George R.	*Elementary Chemistry*		1883	x							
Frankland, Edward & Japp, F.R.	*Inorganic Chemistry*		1884	x	x	x		x			
Anderson, J.H.	*The Public School Chemistry*		1885								x
Maybury, A.C.	*The Student's Chemistry. Part I*		1886	x		x				x	
Shenstone, W.A.	*A Practical introdution to Chemistry*		1886								x
Taylor, R.L.	*Chemistry for Beginners*	1	1887	x							
Muir, M.M. Pattison & Slater, C.	*Elementary Chemistry*		1887	x							
Jago, William	*Inorganic Chemistry Theoretical and Practical*	9	1888								x
Meyer, Lothar	*Modern Theories of Chemistry*		1888								x
Mixter, William G.	*An Elementary Text-Book of Chemistry*	2	1889								x
Bloxam, Charles L.	*Chemistry Inorganic and Organic*	7	1890	x							
Ward, Robert Avey	*Elementary Chemistry for Science Schools*		1890	x							
Ramsay, William	*A System of Inorganic Chemistry*		1891	x							

				Attribution								
Author	**Title**	**Edition**	**Date**	C	W	L	C>W	C=W	W>C	L>C	C>L	None
Ramsay, William	*Elementary Systematic Chemistry*		1891	x								
Fisher, W.W.	*A Class Book of Elementary Chemistry*	2	1891	x								
Watts, W. Marshall	*A Practical Introduction to the Elements of Chemistry*		1891	x								
Luff, Arthur P.	*A Manual of Chemistry*		1892									x
Lilley, H.T.	*A Lecture Course in Elementary Chemistry*		1892									x
Furneaux, William S.	*Elementary Chemistry. Inorganic and Organic*		1892									x
Cox, E.J.	*The Standard Course of Elementary Chemistry*		1892									x
Taylor, R.L.	*The Student's Chemistry.*		1892	x								
Briggs, William	*A Synopsis of Non-metallic Chemistry*		1892									x
Roscoe, Henry	*Inorganic Chemistry for Beginners*		1893	x								
Mills, John	*Chemistry for Students*		1893	x		x						
Harrison, W. Jerome & Bailey, R.J.	*Chemistry for All*		1893	x								
Snaith, W.A.	*Inorganic Chemistry for Elementary Classes*	23	1894	x								
Cooke, Samuel	*First Principles of Chemistry*	6	1895	x								
Sexton, A .Humboldt	*Elementary Inorganic Chemistry*	4	1895									x
Clowes, Frank	*A Treatise of Practical Chemistry*	6	1895									x
Trotman, S.R.	*Elementary Non-Metallic Chemistry*		1896	x								
Perkin, W.H. & Lean, B.	*An Introduction to the Study of Chemistry*		1896	x		x					x	
Vernon Harcourt, A.G. & Madan, H.G.	*Exercises in Practical chemistry*	5	1897									x
Muir, M.M. Pattison	*A Course of Practical Chemistry*		1897									x
Jones, Chapman	*Practical Inorganic Chemistry*		1898									x
Bailey, G.H.	*Advanced Inorganic Chemistry*		1898	x								
Newth, G.S.	*A Text-Book of Inorganic Chemistry*	7	1899	x								
Willson, Rivers	*Chemistry. For the use of Students …*		1899									x
Roscoe, Henry E. & Harden, Arthur	*Inorganic Chemistry for Advanced Students*		1899									x
Ramsay, William	*Modern Chemistry. Theoretical*		1900	x								
Shenstone, W.A.	*The Elements of Inorganic Chemistry*		1900	x								

Key:
C Cavendish W Watt L Lavoisier > favoured over = equal to

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Note: The nineteenth-century chemical texts forming the basis of the survey in Chapter 11 are included here only if they are individually discussed in my text and/or notes. For details of the rest see the Appendix.

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